





AGRICULTURAL RESEARCH INSTITUTE

PIISA





PHILOSOPHICAL  
TRANSACTIONS,  
GIVING SOME  
ACCOUNT  
OF THE  
Present Undertakings, Studies, *and* Labours,  
OF THE  
INGENIOUS,  
IN MANY  
Confiderable Parts of the WORLD.

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VOL. LVII. PART I. For the Year 1767.

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L O N D O N :

Printed for I. DAVIS and C. REYMERS,  
Printers to the ROYAL SOCIETY,  
*againſt Gray's-Inn Gate, in Holbourn.*

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M.DCC.LXVIII.



# ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

## ADVERTISEMENT.

should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of nature or art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons, through whose hands they receive them, are to be considered in no other light, than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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# PHILOSOPHICAL TRANSACTIONS.

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Received in 1765.

- I. *A monstrous human Fœtus, having neither Head, Heart, Lungs, Stomach, Spleen, Pancreas, Liver, nor Kidnies. By Claude, Nicholas Le Cat, M. D. Professor and Demonstrator Royal in Anatomy and Surgery; perpetual Secretary to the Academy of Sciences at Rouen, F. R. S. &c. Translated from the French, by Michael Underwood, Surgeon to the British Lying-in Hospital, in London.*

Read December 1766,  
and January 1767.

**M.** F\*\*\*\*, a chair-woman, in the parish of Carville de Dernetal, aged thirty-four years, was brought to-bed, at nine months end, on tuesday the third of january, 1764, of two children, having already had six.

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This last labour, in which she was attended by the widow *Mauger*, a midwife of the same town, began with so considerable a discharge of water, that it was judged, not without reason, that her pregnancy was attended with a dropfy of the *uterus*.

The first child was a girl, well formed, who died in the birth, solely from the obstacles which were occasioned, during the course of the labour, by the second child, or monster, which I am going to describe.

All the lower part of this child, from a finger's breadth above the navel, was likewise a female, tolerably well formed, except that on her left foot she had but four toes, joined together by a membrane, like the web of a duck's foot.

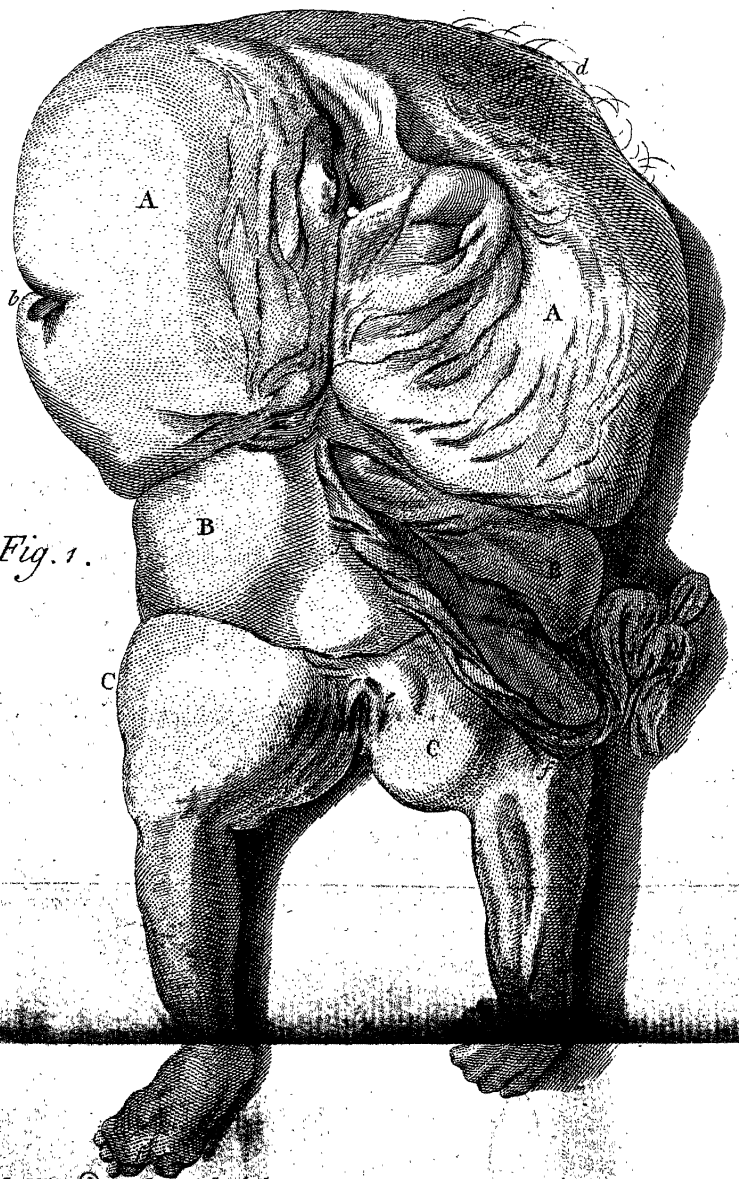
But all the parts of this foetus, above the navel, composed a perfect *mola*, a shapeless mass, represented in the two following figures, of which it will be necessary to read the explanation, in order to have a just idea of the external appearance of this monster.

The drawings are reduced to almost half the natural size, both as to length and breadth; from whence it may be observed, that each of the children, who had lived to their full time, had acquired, in this dropfical womb, a bulk pretty near equal to that of other children, born at full time, when twins, and not very thriving. The full size of our monster was twelve inches six lines, and the navel was in the middle of this space.

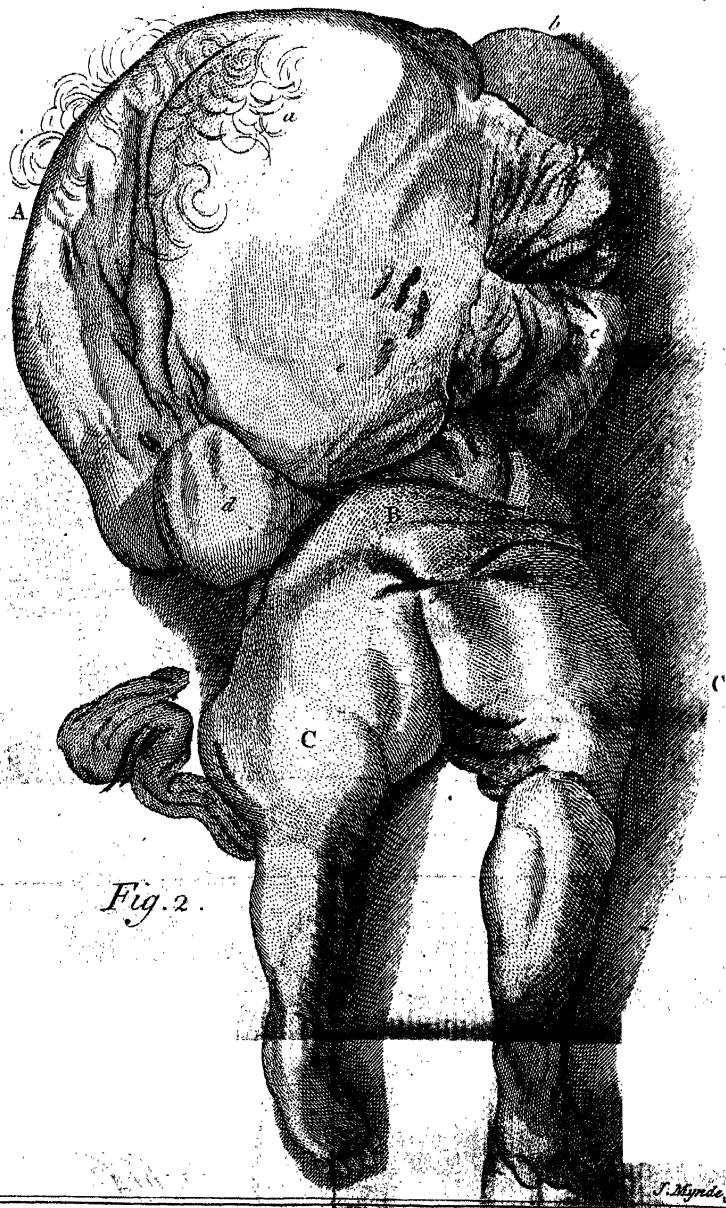




*Fig. 1.*



*Drawn by M<sup>rs</sup> Davis daughter to the Author.*



*Fig. 2.*



# [ 3 ]

## T A B. I.

### FIGURE 1.

Represents the monstrous foetus [in a front view.

A. The upper and only monstrous part, which seems to contain its head, its superior extremities, and the breast.

B. The belly.

C. The lower extremities.

a. Solid and bony eminences, which appeared to be portions of a jaw, without any opening for a mouth.

b. The extremity of the thumb of the right hand.

d. The hair, the same as on other children.

All the surface of this bag was like the skin of any other foetus.

e. The genital-parts of a female.

f. The *umbilical* cord.

### FIGURE 2.

Represents a back view of the foetus.

A. The same bag as at letter A. fig. 1.

B. The loins.

C. The lower extremities.

a. The hair as seen in the former Fig. under which might be felt the bones of the *cranium*.

- b.* A transparent vesicle, like an *bydatide*, which I took for an imperfect eye.
- c.* Part of the bag, where we observed the extremity of a thumb seen at *b*, fig. 1.
- d.* Part of the same bag, which resembled that marked *c*.
- e.* Under this region might be felt the *spina dorsæ* and *colli*.

### Description of the internal parts of this monster.

I began the dissection on the hind part; the muscles of the back were well formed. I found on the right side eight ribs, reckoning from below upwards, and seven on the left. Immediately above these was an *bydatide*, in which lay the cervical nerves, destined for the upper extremities. In this place also might be seen very imperfect rudiments of the *scapula* and *clavicle*.

On the right side, and near to those rudiments, was a sort of thumb, easily known to be the thumb of the right hand, whose extremity projected beyond the integuments three-fourths of its natural length.

Above, and on the right side of this *bydatide*, I discovered another more considerable, surrounded by a large bag, very smooth on the inside, and supported by something, which had the appearance of a beginning of *maxillæ*.

At the extremity of the fore part of this bag were two orifices, almost contiguous, across a *septum*, which led to another bag of a much smaller size.

These two *hydatide* bags were behind that represented in Fig. 2. (letter *b.*) which I took for an imperfect eye, because it was transparent, and surrounded by teguments not unlike eyelids.

On tracing the *spine*, and divesting it of all the soft parts, we discovered that it terminated above in a bony mass, that resembled the *larynx*, above which was a large soft substance of the consistence of, and covered with, that kind of skin common to a cow's udder; on which we saw hair like that of other children. This occupied the usual place for the head.

Under this kind of *parenchymatous* substance, which was white and glandular, was a muscular mass, more considerable and conspicuous than one could well have expected in such a subject. It doubtless consisted of the *occipital* and perhaps *frontal* muscles, drawn towards each other.

Having raised this fleshy part, I opened the upper bag of the *spine*, resembling a *larynx*. Its surface was altogether bony, as usual in the foetus, viz. somewhat cartilaginous. This I opened in the direction of a membranous triangular line, somewhat like the *lambdoidal suture* of the *occiput*. We found this kind of *larynx* fitted with *cerebrum*, or rather *cerebellum*; it might be about a cubical inch in proportion; and this was all the brain of the foetus.

At the extremity of this cavity, backwards, lay the proper *medulla spinalis*. This cavity was not separated by an elongation of the *cerebellum*, it had but a very small *falx* forwards; and on the right side was another appearance of an elongation of the *cerebellum*;

*cerebellum*, so that this small portion of brain did most likely belong to the *cerebellum*.

At the basis of this kind of unformed *cranium*, forwards, was an opening leading to a small brown *hydatide*, situated on the right side, under a bone which had the appearance of a portion of the *maxilla*, which led towards a sort of mouth, scarce formed, and closed; it is represented at the letter (*a*) of the first figure. There was nothing on the other side, no appearance of a mouth, nor any thing that seemed the least like it.

I took this *hydatide* for an unformed jugular bag, or true *cæcum*; in the adjoining bone I found a kind of right ear.

The fore part of this same superior surface of the *cranium* was flat, but a little hollowed, like the upper surface of the *larynx*; in the middle was a considerable ridge, and on its anterior part appeared a prominence: thus this bone, which should have been similar to the two *parietals*, did not resemble them at all.

Underneath this prominence, the bone took a perpendicular turn, making a sharp angle with the upper surface, and forming a cavity in its descent, which terminated in a projection forwards; it was on the right side of this projection where the supposed right branch of the *maxilla* was attached; within that branch appeared the trace of the jugular above-mentioned, and very distinctly the nerve of the eighth pair.

In the breast, or rather under the ribs, were neither heart nor lungs, but the same white, *pneumatous* and *œdematous-like* substance, which we

saw in the place of the head. Below this was no *diaphragm*, at least no distinct one. In the belly, which extended itself just under the ribs, was a bundle of intestines, and a little red mass, which I called the liver, for want of a better name, because it seemed, that, when I pulled the umbilical cord, this substance moved, which induced me to believe that the umbilical vein entered there.

No stomach, spleen, pancreas, or kidneys were seen.

The intestinal mass was divided into two portions. The first was of a reddish colour, which terminated upwards in a blind pouch, and below joined the other portion, as the *ileum* does where it unites with the *colon* and *cæcum*. This second portion was white, and seemed to include the large intestines. The *cæcum* was very long, or rather the *cæcum* and its *appendix vermi-formis* were of the same size.

Thus, there was neither *jejunum*, or *duodenum*, or stomach, or any liver properly speaking: for that, which I found in the place of it, was a red *viscus*, and of the *conglomerate* kind, like the kidney in a *fœtus*. Having cleared it from all its adhesions, I discovered neither vessels analogous to those of the *sinus* of the *vena portæ*, or any thing that resembled the figure of the liver, or of any of its appurtenances. I opened it, and was more and more convinced that it was rather a kidney, or knot of renal glands, than a liver, although it was one mass, and placed in the midst of the intestines; it had still less the resemblance of a heart, having no cavity, no vessels, or any muscular fibres.

The



The extremity of the *colon*, or the *rectum*, passed betwixt the bladder and the *uterus* as usual. I dissected all these parts, and traced the bladder up to the umbilical cord, where it lengthened into a pipe, and formed an open *urachus*. It had not the pyriform shape of the common bladder.

It was in making these dissections of the *kidney-liver*, and those of the *pelvis*, that I divided the principal vessels, which I shall mention in the second examination.

All these intestines, and especially the *rectum*, contained excrements of a light ash colour, but no *meconium*.

The bladder, although lengthened and continued by an *urachus*, as high as the navel, opened in the usual place betwixt the *nymphe*.

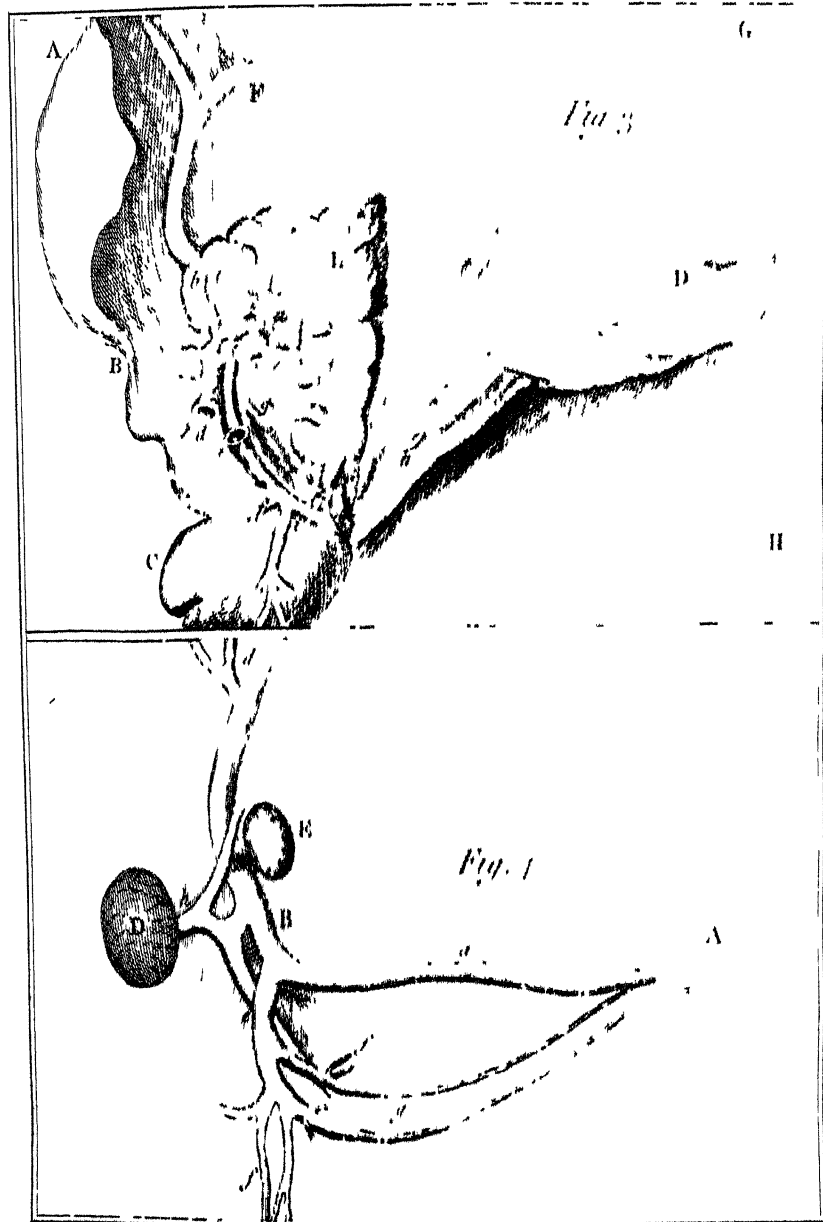
The *anus* was imperforate, and the *rectum*, immediately under the *uterus*, terminated in a blind pouch, attached to some membranes that went to the *anus*. This pouch was quite full of that sort of excrement just now described.

Second examination, made the next morning.

I had confined myself, in the first examination, to the singularity of a want of head, lungs, and heart, and of the existence of nerves, notwithstanding these defects. In this second review, the organs of the circulation in such a production, which had lived nine months, raised my curiosity; but it was rather too late for my intire satisfaction, for I had not taken care enough to preserve the internal parts, imagining before-hand that they resembled those  
of









of two headless twins, in my possession, whose inside was intirely simlar to that of other fœtuses. I discovered, notwithstanding, the following things, and I have represented the most essential in a rough sketch seen in figure 3.

### FIGURE 3.

A. Is the region of the breast, whose internal surface was lined with a membrane, which I took to be a *diaphragm* thrust back and stuck to the *pleura*, because it arose from lumbar muscular portions resembling the pillars of the *diaphragm*.

Under this species of midriff was a very regular distribution of arteries and nerves, which I neglected to delineate, as thinking it useless; I contented myself with dissecting the *aorta* F. of which the upper ramifications were as usual.

B. is the lumbar region.

C. Those of the *ossa ilia*, and of the *pevis*.

D. The *umbilical* cord passing through an opening across the teguments of the lower belly, to be brought into view.

E. The intestines.

F. The *aorta superior* before-mentioned.

G. H. The integuments of all the right side, opened on the back, to preserve it whole, and also those of the belly; they are thrust to the right side to give a view within.

- a. A kind of single kidney, which occupied the place of the liver. The principal part is covered by the intestines. It received several small vessels from the *aorta*, but none of them was near so large as the *emulgent*.
- b. A small lobe, that might pass either for a small lobe of the liver, or for a *capsula renalis*.
- d. An orifice of a vein, which was also ramified, through the *kidney-liver*, by branches as fine as those it received from the *aorta*. But this vein did not terminate there.

1. It sent, upwards, branches to the muscles, to the *vertebræ*, and thence to the ribs; for, by blowing into the trunk, the air came out at the origin of the uppermost rib I had dissected.

2. Below, it formed two large *iliac* veins, g. which took the usual course.

- e. The trunk cut, belonging to the *aorta inferior*, of the length of a line.
- f. The distribution of the rest of this *aorta*, or right *iliac*, through that side of the *pelvis*.
- g. The *iliac* veins going off to right and left, as usual.
- h. The orifice of the *umbilical* vein, being so exactly of the same diameter, of the same white colour and strength, and of the same consistence with the divided trunk, e. that at first sight, I took this for the same vessel. The trunk, d. was much thinner, and of a more bluish tint.

Under the *umbilical* vein, *b.* was the *umbilical* artery on each side, which went as usual into the *pelvis*, and there sent off the ordinary ramifications.

On the right side, its *anastomosis* with the *iliac* artery was very discernible; but on the left we could not discover the *iliac*, a vessel which is pretty considerable, even in the *fœtus*, and was so on the other side, as seen at letter *I.*

On the left side, the *umbilical* artery, at its origin, or its inflection, had a kind of web of arteries subdivided, some of which, doubtless, communicated with the *aorta*, or right *iliac*; but neither of these branches appeared to be near the size of the right *iliac*, nor could we find any thing any-wise like to the trunk, *c.* which I long suspected to be the trunk of the right *iliac*.

What vessels then are these trunks, *d. e.*? Which of the two is the continuation of the *umbilical*, *b.*? This is a very important point, but not easily determined. It would not have proved so, if I had injected the *umbilical* vein, as I generally do in all my monsters; or if I had taken more care of the parts about the *kidney-liver*, which I certainly should have done, had I but suspected so many singularities.

I said, that at first sight, I took the trunk, *c.* to belong to the *umbilical* vein, and it is still an opinion, to which I am inclined, for the following reasons.

1. At this part, *c.* the *aorta* had its greatest circumference; and, in tracing it from this trunk, above as well as below, it grew less and less. This was, therefore, its trunk, or origin, and could not be a branch of it.



2. I have already said, that the mouths, *b.* of the *umbilical* vein, and, *e.* of the *aorta inferior*, were exactly of the same diameter, of the same white color, and of the same strength; and that the other vessel, *d.* was much weaker, of a laxer texture, and of a somewhat livid color, like the coats of the veins.

3. The *umbilical* vein is, with respect to the mother, or to the *placenta*, which transmits the blood to the *fœtus*, a real artery, going from the center to the circumference, or from the principal body, which is the mother, to an adjoining organ, which is the child; and the *umbilical* arteries are properly veins, which return the blood from that adjoining body to the common center of the grand circulation. The blood from the *umbilical* vein then is truly an arterial blood to the *fœtus*. In the usual structure of the *embryo*, nature has shortened all the ways, to bring the arterial blood of the mother more speedily into the heart, into the very *aorta inferior* of her *fœtus*. Therefore, in a subject, where there is no heart, or even liver, that vein ought to communicate immediately with the *aorta inferior*. In this manner one conceives how this subject could do without a heart, the *umbilical* blood being a continuation of that from the arteries of the *placenta*, the *uterus*, and in short of the mother; the impulsion of the maternal blood was propagated by that *aorta* through all its ramifications both above and below. In one word, the heart of the mother supplied that of the *fœtus*, and the circulation in this was a continuation of that of the mother. These are the reasons inclining me towards that first opinion; and here are those that suspended my judgment for some time in favor of the supposition  
of a

of a communication of the *umbilical* vein with the trunk, *d.* of the *vena cava* of the fœtus.

1. In every fœtus the *umbilical* vein empties itself into the *vena-cava* in the liver; therefore nature hath here followed her usual course.

2. With regard to the arterial trunk, *e.* it is close by the division, *g.* of the *Iliac* vein, whence it is very likely that it was the left *iliac* vein which was divided.

This last, and above all striking reason, made me employ almost a whole morning, in looking over and over this left *iliac* region, to discover the divided vessel, which would have put the whole matter out of dispute; but I could find no trace or appearance of it. All the vessels communicating with the left *umbilical* vein appeared very intire, though deprived of part of their ramifications by which the air escaped, but all grew in their course less and less in diameter. Which then was the origin of this left *umbilical* artery? doubtless the branches of the trunk, *f.* of the *aorta*, which were numberless in the *pelvis*, but had partly been spoiled the preceeding night, in dissecting the *rectum*, *uterus*, and bladder of this monster. I add, that this trunk, *e.* was joined to some membranes, which we were obliged to pull about, in order to make it turn to the left, and this direction appeared not to be its natural position.

Be it granted, for a moment, that, *e.* is the left *iliac*, and that the *umbilical* vein joins it at, *d.*; how could the blood circulate in this fœtus? How could it have lived the nine months? *d.* is evidently a trunk of the *cava*, which generally enters the right auricle of the heart, dividing, like this, into the

*cava superior*, *d. b.* which rises by the *vertebræ* up to *F.* and into the *cava inferior*, *d. g.* 1. It would be absurd to place the only moving power of circulation in the *vena-cava*, or indeed in any vein. 2. When you have placed it there, what will this supposition tend to? 3. This vein subdivides, and ramifies itself through the *kidney-liver*, the muscles and the spine; but none of its branches communicates with the *aorta*. The *aorta* on its part sends several branches into the *kidney liver*, very slender, and resembling, by their transversal direction, the common emulgents, but very different in size. If then the circulating force were placed at *d.* it could only produce an inverse circulation, by the communication the *cava* might have by its capillary branches, with the like ramifications of the *aorta*, which supposition seems too much against nature to counterbalance the other opinion, which makes the trunk *e.* of the *aorta*, a portion of the *umbilical vein*, and the substitute of the heart.

Another anatomical fact proves this last opinion; which is, that the *aorta*, and especially the superior, *F.* ran up as high as the *cranium*, and was of a pretty considerable size, while the venal trunk, *d.* had nothing but capillary branches in the upper parts; so that it almost appeared evident that the vessel, *e. F.* was connected with the chief mover of the fluids. Wherefore, supposing the trunk, *d.* to be the *vena porta*, or an imperfect *cava* going to some of the *viscera*, being the rudiments of an imperfect heart, or a *vena-cava* ending in a pouch, as the intestines did which should have entered the stomach, if there had been one; the difficulty almost disappears. I say almost, because, even on this supposition, if there was a circulation

tion in this monster ; we must admit some *anastomoses*, between the arterial and venal system, which supplied those found in other foetuses ; since the venal blood must in some place or other re-enter the arterial torrent. Such might be the *anastomosis*, K. fig. 4.

For, by this hypothesis, the vascular system of this subject would be represented by fig. 4. in the following manner.

- A. The *umbilical* cord.
- B. The intestinal tube.
- D. The kidney-liver.
- E. A sort of *glandula renalis*.
- a. The *umbilical* vein ; the great mover of the fluids.
- b. The *aorta*, a continuation of that vein.
- c. d. *aorta superior*, accompanied by the *vena-cava*.
- e. *Aorta inferior*.
- f. The distribution of the *iliac*.
- g. The *umbilical* arteries, making a part of the distribution.
- h. The trunk of the *venae-cavae* coming either from the *portae*, or from the *viscus* D, or forming a blind pouch in that part. Some traces of the *vena-cava superior* appear towards C.
- I. The *cava inferior* going to form the *iliacs*.
- K. A necessary *anastomosis* between the two kinds of vessels, arteries, and veins. I place it in this conspicuous situation, though it might have been any where else.

I repeat

I repeat it again, if I had but suspected so many singularities, what I now can give only by way of conjecture, might have become demonstrable in fact. It is scarce probable that I shall ever have such another opportunity; but it is more so, that it may offer to some one among the great number of the literati in Europe, who read the Philosophical Transactions. This was the principal motive that determined me to present this observation, though imperfect, to the Royal Society. Why should we hesitate to make a publick acknowledgement of our faults, when our brethren may profit by, and amend, them?

Another motive, which engaged me to offer this observation, such as it is, was, that even the imperfection of it does not affect the useful consequences deducible from it: for, whatsoever may have been the disposition of the blood-vessels of this monster, it is a fact absolutely certain, that it had no heart, nor any other *viscus* in the place of it; and that the circulation of the fluids, which appears to have taken place from the existence of the principal arteries and veins, could not have had any other moving power than the circulation of the mother itself. Hence this child, monstrous as it is, demonstrates the circulation of the blood from the mother to the foetus, and from the foetus to the mother again; which some moderns deny, and others endeavour, at least, to render doubtful. I presented to the Academy at Rouen some years ago, several observations which favoured the antient system; the present comes to their support, to give this excellent hypothesis of Harvey all the credit it deserves.

The

The child I speak of had no mouth, *oesophagus*, nor stomach, thus it could not, by that usual passage, be nourished from the waters that surrounded it; it could not absorb from the surrounding fluid wherewith to fill its vessels, and supply its growth. It, therefore, follows that it received both its arterial and alimentary fluids from the mother by the *umbilical* cord, and that it owed every thing to that circulation, which some would attempt to annihilate.

In the intestines of new-born children we find a black excrement, called *meconium*; this black pulp can receive its color only from a bile thickened by retention, and poured directly from the *ductus choledochus* into the *duodenum*. Now this foetus, having no liver, nor gall-bladder, &c. could have no *meconium*; therefore the pulp found in the intestines was of an ash-colour.

This monster had so little brain, that that *viscus* must have been of small import towards the functions of this animal. And yet all these brainless foetuses are very lively. Mr. Denis, who, in his twelfth conference, has given an account of one of them, and M. Vaisiere, who sent me one from Toulouze, the last year, both remark, "that these children  
" are remarkably lively in the mother's belly;  
" that they were in violent motion, at the time  
" of labor; that the moment they were in the  
" world they seemed suffocated, and became all at  
" once motionless." This is a matter worthy of much reflection.

How can we conceive there can be sensation and motion, without almost any brain in one of these monsters; and absolutely without any in the other?

Sensibility, sensations, and passions, may exist without the brain, and have their seat in its *meninges*, and in the coats of the nerves formed by those *meninges*. If I had not endeavoured to prove that point in my physiology, and lately in my dissertation on the sensibility of the *dura* and *pia mater*, the observations of children and animals born without brain, which are pretty considerable in number, would demonstrate it by facts.

That these children are more lively, that is, more sensible, I attribute to their having little of the nervous juice, though not less of the active fluid. This nervous juice I termed *fluide conservateur*, the preserving fluid, in my physiology. It is long since I have observed that an abundance of this nervous juice produces the contrary of vivacity; and, as a consequence, of our principles, long sleep, or rigid continency for a length of time, renders us heavy and benumbed, because in each of these circumstances, this nervous juice abounds and regurgitates, if I may so speak.

But these violent motions, whence have they their origin? there must be a nervous juice, to act in the muscles, and here we have very little.

Neither of these animals was deprived of the *medulla spinalis*, and one of them had a small portion of brain, or *cerebellum*. This is one source of the nervous juice, and of the active fluid, necessary to muscular motion. This source, I grant, is weak and poor, but I have made it appear in my treatise on this subject, which obtained the prize from the Academy of Berlin, that there is in the blood a richer store, which the nervous fluid unites to, and

makes use of, in muscular motion. We explain by this, how it happens that an ass, who has so much less brains than a man, is, notwithstanding, so much stronger, because it has much more blood.

Here then is a second spring that affords these monsters a considerable supply; but though sufficient for their motions, it is not equal to that of an ordinary foetus, and the violent agitations of their body arise from their great sensibility, which we have just now accounted for. Now, the blood in the foetus, and especially in these, belongs to the mother; they are furnished by her, as well with air, as with the nervous juice, and the animal fluid, which are essential to her. Wherefore, as soon as these children are separated from the mother, and deprived of that vital source, all motion must cease in them, as if they were suffocated, that is, as in any other suffocated foetus.

Let us conclude this account by a word or two on the cause of these monsters.

The great quantity of waters voided by the mothers of these children, proves that the principle of their monstrosity is a disease, a sort of dropsy, and even a kind of *hydrocephalus*, which had run off a considerable time before the labor.

The two *hydatides* I found at the origin of the *brachial* nerves, and which had evidently been the cause of the mutilation of the upper extremities, are examples that help us to comprehend that of the other organs. On supposing a like disorder on the origin of other nerves, which have their rise from the brain, it will be obvious that the organs, to which these nerves run, that is, where they convey the nervous fluid,



fluid, which contain the rudiments of every part, will be wanting. It may indeed be said, there are *hydrocephali* that have all the organs very well formed: but there the disease has commenced after the perfect formation of these parts, whereas if you suppose it to have happened in the very time of that formation, you will see that the nervous juice, vitiated, diluted, and turned out of its natural course, can no longer be employed in the generation of those organs.

Those who have attended my courses, and have read my physiology, will be pleased to see that all these mysteries, which one would have thought impenetrable, are easily accounted for upon the principles I have laid down.

Received October 26, 1766.

II. *A Letter to Dr. Watson, F. R. S. containing a Description of Three Substances mentioned by the Arabian Physicians, in a Paper sent from Aleppo, and translated from the Arabic, by Mr. J. Channing, Apothecary.*

S I R,

Read Jan. 8,  
1767.

**A**T your desire, I send you the translation of the Arabic, and the specimens which you saw at my house. The passages included within hooks, with an asterism before them, are added, to make it more intelligible. I send likewise a copy of the paper which came with the specimens, written by a gentleman of the factory at Aleppo. It will give me pleasure, if they should be thought worthy of the inspection of the Society.

You will easily guess at my view in procuring these specimens. The Tabashir, Mamithsa, and Mammiraan are used by the Arabian physicians; by Rhazes particularly; in page 62 (not. 32) page 110 (not. 4); page 146 (not. 6). I have given the best account of each, which I could meet with; and you will see it differs not much from this paper, which came after that book was finished and printed off.

Last.

Last week, in the Bodleian Library, I met with an Arabic MS. of Dioscorides. It appears quite intire and perfect. The Greek titles are inserted in the margin by Dr. Hyde, in red ink, which is some proof of his value for this MS. To me it appears a real treasure, and it is likely it may be of excellent use in correcting the very corrupt text of that author: perhaps too it may be a means of ascertaining the Materia Medica of the elder Greek physicians. The Escorial MS. contains only the first three books, and is imperfect at the beginning. If a transcript of this latter, however, could be obtained, it might likewise be of great use.

I am,

S I R,

Your most obedient

humble servant,

Effex-street,  
Oct. 24. 1766.

John Channing.

*Copy of a Paper sent with the Specimens, by a Gentleman of the Factory at Aleppo.*

THE specimens sent of the Tabasheer, though taken from different parcels, are not regarded here as different sorts. Amongst them will be found one or two pieces, which in their form answer to the ancient Arabic description of this drug. It is not from the sugar cane that the Tabasheer is supposed to be procured, but from that kind of cane of which the Arabs make their lances, and of which a piece is herewith sent.

Different opinions concerning this substance, as also concerning the Mamithsa and Mameraan, collected from sundry Arabian writers, will be found in the paper inclosed: but as the Tabasheer is brought from the East, not prepared here, I cannot affirm any thing certain about it.

From the specimens sent of the Mameraan, it will appear evidently to be a root. It is commonly believed to be a species of the Chelidonium, and, like the Tabasheer, brought from the East to Aleppo.

Mamithsa, ماميثسا or ماميتا, is the common name used here for wormwood. Our mint is called Nana نناع. The literary name, however, of wormwood is افسنتيون (\* Ifsantin, absinthium).

But there is a plant here known by the name of ماميتا (\* Mamitha) of which a specimen is now sent. This, from the use made of it by the natives in distempers of the eyes, as well as from other circumstances, appears to be the Glaucium of Dioscorides.

Dioscorides. An *Papaver corniculatum floribus cæruleis*?

Concerning the *Ibbidrowia*, I have been able to get no intelligence. The *Orichalcum* is called here طاج or جام (\* *Dgaam* or *Tûck*).

The paper made of silk husks is not to be found at present in the city. If any can be procured from the *Bassora* caravan lately arrived, it shall be sent.

*Translation of a Paper in Arabic, sent to W———  
C———, Esquire, from Aleppo, with several  
Specimens of Tebashir.*

طباشير, with a طاء. Tebashir.

In the *Camus* (\* an Arabic Lexicon, which the celebrated *Golius* translated into Latin), *Tebashir* is said to be a substance found in the hollow of that species of Indian cane of which lances are made: or the lower part of that cane burnt. The *Tebashir* which is formed at the knots of the cane is round like a *dirhem* (\* *ducat*). This substance is found in the cavity of those canes which have been fired by rubbing one against another. It is frequently adulterated: the burnt bones of sheep, the skulls particularly, are sold for it.

*Ebn Beitar*, in his treatise of simples, says, “*Tebashir* is a substance found in the hollow of the Indian cane.”

*Ali Ebn Mohammed* says, “It is the burnt part of the lower stem of the Indian cane; and is imported only from the coast of India; chiefly from that part of the coast called *Sendapour*.” (\* or

“ (\* or Sendafour) from whence the black pepper  
“ is brought.”

Avicenna, in his Canon, says of Tebafhir, “ it is  
“ the lower part of the cane which has been burnt ;  
“ it is reported that the canes are fired by being  
“ rubbed one against another by the violence of the  
“ winds. This drug is produced on the coast of  
“ India.”

### الماميثا Mamitha.

Ebn Beitar in his Treatise of Simples calls it Mamitha. Abu'l Abbas the Nabathæan (\* the botanist) calls it Mamithia. (\* i. e. the letter is written sometimes with 3 two, sometimes with 3 three points over it). Both these names are sufficiently known.

In the treatise of simples called Ma-la-Yesâ (\* i. e. a treatise of those things which no physician ought to be ignorant of ) ماميثا Mamithia, is “ the name  
“ of a plant like the papaver maritimum, or corniculatum. At the lower part of the Mamithia is a  
“ moisture which sticks to the hand : it has a yellow flower like the papaver before mentioned ; its  
“ seeds are different, inclining to black, like and  
“ about the size of the seeds of setanum. The  
“ plant is of a strong and offensive smell, and very  
“ succulent. The difference between these two  
“ plants is this ; the papaver corniculatum dies to the  
“ root in the winter, and sprouts again from its root  
“ in the spring ; the Mamithia, on the contrary,  
“ sprouts again in the spring from the top of its stem.”

Avicenna, in his Canon, says, “ Mamithia is  
“ like acorns, of a yellow colour inclining to black,  
Vor. LVII. E easily

“ easily broke. It is bitter, of a substance watery  
 “ and earthy; cold, but not vehemently so; its  
 “ juice is in the same degree of cold as the wa-  
 “ ter of pools or lakes. It is prepared from a plant  
 “ which is brought from Manbedge” (\* a town of  
 note in Syria, vid. Geogr. Nubiens. page 120, line 7,  
 and Index Geograph. in vitam Saladini, in voc. Man-  
 besjum) “ of a very diffusive scent, a bitter taste,  
 “ whose juice is yellow, of a saffron colour.”

Mamiraan. In the Liber Memorialis, it is said,  
 “ Mamiraan is a plant, at the bottom of whose  
 “ stem are produced knotted, crooked, hard roots.  
 “ The Indian is the best; this inclines to a black  
 “ colour: the Chinese to yellow: the other sorts are  
 “ green. It grows in the water; its leaf is like  
 “ the leaf of the convolvulus; it is hot and bitterish;  
 “ its seed is like that of sesamum.”

It is said in the Canon of Chalid and Manown,  
 “ some say it is a root, and called Mamiraan; others  
 “ say, the smaller roots are called Mamiraan, but  
 “ the larger Zeradgush” (\* in Castell’s Lexicon, col.  
 308, and in Meninski, col. 2441, the word is  
 زردجوب Zeradgiob, which signifies yellow wood,  
 and is the Persic name for curcuma).

Avicen, in his Canon, says, “ Mamiraan is a  
 “ woody knotted substance, inclining to a black  
 “ colour, has small curvatures, and is one of the  
 “ things used by dyers.”

Ma-la-Yefa says, “ Absinthium is a Greek word,  
 “ in Persic it is called Mowi Chowshch. This is a  
 “ plant which grows freely and largely; it rises in  
 “ a stem, from which shoot out many branches,  
 “ on

“ on which are many thick and tufted leaves; it  
 “ bears a flower like that of parthenium, small and  
 “ white; in its middle it has a part yellow; its head  
 “ is small, in which is a small seed; its taste is bit-  
 “ ter and styptic. Some sorts of it have a leaf like  
 “ the daucus, and a yellow flower. The inhabitants  
 “ of Egypt call this kind of it Demsisah. It grows  
 “ plentifully in the East, and in Syria, Chorasan, and  
 “ Irak. The two last sorts of it are less esteemed,  
 “ and of less value.”

Absinthium “ some physicians call this Alsbich  
 “ Alroumi” (i. e. Absinthium Ponticum, or Ro-  
 manum).

Look into the Canon of Avicenna, under the ar-  
 ticle Absinthium, you will find there several things  
 concerning Mamithsa. Consider that article, there-  
 fore, very attentively.



Received January 8, 1767.

III. *A general Investigation of the Nature of the Curve, formed by the Shadow of a prolate Spheroid, upon a Plane standing at right Angles to the Axis of the Shadow; in a Letter to the Royal Society, by Mr. George Witchell, F. R. S.*

Gentlemen,

Read Jan. 15, 1767. **I** Beg leave to lay before the Royal Society the following investigation of an irregularity in the duration of the eclipses of Jupiter's satellites, occasioned by the figure of his body.

It has been known for a long time, that Jupiter's body was not truly spherical, but a prolate spheroid, ~~and that its greatest degree~~ *and that its greatest degree* ~~than any of the other planets; but notwithstanding this,~~ *than any of the other planets; but notwithstanding this,* it was never suspected that it would affect the durations of the eclipses of the satellite, till Dr. Bevis first thought of it, in the latter end of the summer 1761.

The Doctor, being at that time indisposed, recommended the subject to my consideration; and, in compliance of his request, I not long after presented him with a solution of the problem, being in

substance the same with this, as far as proposition V. a copy of which he soon after transmitted to that excellent mathematician the late M. Clouaut.

In March 1763, M. de la Lande, an eminent French astronomer, being here, Dr. Bevis shewed him my paper; this occasioned a new article in the *Conn. des Mouv. Celsst.* 1765, p. 177, under the title, *Inégalité dans les demi-durées des éclipses des satellites de Jupiter, causée par l'applatissement de Jupiter*: in which he mentions this circumstance in the following words; “ M. le docteur Bevis me fit voir à Londres, au mois de Mars dernier, une solution rigoureuse & algebrique de ce probleme, qui consiste à trouver la courbe qui résulte de la section de l'ombre d'un spheroides à une distance quelconque.”

In this state it remained ever since; for though the Doctor, and some other gentlemen, to whom I shewed it, frequently urged me to lay it before the Royal Society; I always declined it, till I should have time to make some farther additions to it.

A few months since, M. Bailly, a French gentleman, published at Paris an elaborate treatise upon the theory of Jupiter's satellites; in which he has been pleased to give the honour of this discovery intirely to M. de la Lande, without the least mention of Dr. Bevis. I then thought it incumbent on me to do justice to the Doctor, by immediately finishing my paper in the best manner I was able, and presenting it to the Royal Society.

“ I shall be extremely glad, if this rudeness should excite some more able person to treat the subject in the manner it deserves; for although, I believe, my solution will not be deficient in point of truth, I am not

not vain enough to think it may not be performed in a more elegant manner. I have the honour to be,

Gentlemen,

Your most obedient

humble servant,

Fleet-Street,  
Jan. 7, 1767.

George Witchell.

# LEMM A.

If any spheroid is cut by a plane, in any direction whatever (excepting that which is perpendicular to its axis), the figure of the section will be an ellipsis. This is demonstrated in Simpson's Fluxions, Vol. II. p. 456.

## PROPOSITION I.

TAB. III. fig. 1. Let the sphere BEGK be cut through its center by the planes BGK, BPD, B $\alpha$ D, BOD, EAK, and LPH; it is required to determine the inclination of the planes LPH, BOD, and also the inclination of the right lines AC, BC, which is measured by the arc AB; there being given the angles of inclination EBF, FB $\alpha$ , together with the arc BF: the angles AFB, EAL, being right angles, and the inclination of the required plane BOD, but little exceeding that of the given plane B $\alpha$ D.

Let









Let the sine of  $EBF = a$ , its cosine  $= a'$ , the tangent of  $BF = \beta$ , its cosine  $= \beta'$ , the sine of  $FBA = p$ , its co-sine  $= p'$ , the sine of  $aBA = z$ , the sine of  $AB = Z$ , its cosine  $= Z'$ , the sine of  $PAO = \zeta$ , and radius  $= 1$ ; then we shall have the sine of  $ABF =$  the sine of  $\overline{FBA} + \overline{aBA} = p + p'z$ , and its cosine  $= p' - pz$ : Therefore by trigonometry we shall have in the right angled spherical triangle  $ABF$ , as Rad. (1) : cosine  $BF$  ( $\beta'$ ) :: sine  $ABF$  ( $p + p'z$ ) : cosine  $BAF =$  sine of  $LAB$ , or its equal  $PAO$ ; therefore  $\zeta = \beta' \times \overline{p + p'z} =$  the sine of the required inclination of the planes  $LPH$ ,  $BOD$ . In like manner in the same triangle it will be as rad. (1) : cotan.  $BF$  ( $\frac{1}{\beta}$ ) :: cosine  $ABF$  ( $p' - pz$ ) : cotan.  $BA = \frac{z}{Z}$ ;

hence  $Z = \frac{\beta}{\sqrt{\beta^2 + p'^2 - pz^2}}$ , and  $Z' = \frac{p' - pz}{\sqrt{\beta^2 + p'^2 - pz^2}}$ , which are the sine and cosine of the required arc  $AB$ .

### COROLLARY I.

If instead of a sphere we now suppose  $BEGK$  represents a prolate spheroid, whose axis is  $CP$ ; the figures of the sections  $LPH$ ,  $BOD$ , &c. instead of circles, will become ellipses (by the lemma); but it is evident that the inclinations of those planes to each other, and likewise the inclination of the right lines  $AC$ ,  $BC$ , or the angle  $ACB$ , will remain unaltered.

### COROLLARY II.

If  $BEGK$  represents any primary planet revolving about the sun in an orbit whose plane co-incides with



with the plane BCD, it is manifest that BCD will be its ecliptic, making the angle of obliquity BIE with its equator EAK (whose pole is P); and if B be the place of the sun in this ecliptic, at any given time, the arc BI will be the distance of the sun from the nearest equinoctial point I; and the arc BF his declination at the same time.

### COROLLARY III.

If the plane POG, which passes through P, the pole of the spheroid, be perpendicular to the plane LPH, it will also be perpendicular to any other plane BOD, which passes through A, the intersection of the equatorial plane EAK, with the plane LPH; therefore the angle ACO being a right angle, it is evident that AC will be the semi-transverse, and CO the semi-conjugate axis of the elliptic section BOD.

### COROLLARY IV.

Hence it appears, that the transverse axis of any elliptic section BOD, made by a plane passing through the center of the spheroid, will always be equal to the equatorial diameter of the spheroid, but the conjugate axis will be longer or shorter, according as the inclination of the planes LPH, BOD, is more or less.

### PROPOSITION II.

FIG. 2. To find the length of the semi-conjugate axis CO, of the elliptic section AOb, formed by a plane cutting the given prolate spheroid POG through its center C, and making the angle PCO with the axis CP.

Let

Let the sine of the angle PCO =  $\zeta$ , CG =  $t$ , CP =  $c$ , CO =  $\kappa$ , (radius being unity); draw PO perpendicular to CP: then in the right angled plane triangle BCO, we have as rad. (1) : CO ( $\kappa$ ) :: sine PCO ( $\zeta$ ) : BO ( $\kappa \zeta$ ); and rad. (1) : CO ( $\kappa$ ) :: cosine PCO ( $\sqrt{1-\zeta^2}$ ) : BC ( $\kappa \sqrt{1-\zeta^2}$ ); but from the nature of the ellipsis we have  $\frac{c^2}{t^2} \times \overline{t^2 - \kappa^2 \zeta^2} = \overline{BC}^2 = \kappa^2 - \kappa^2 \zeta^2$ ; therefore  $\kappa^2 = \frac{t^2 c^2}{t^2 - t^2 - c^2 \times \zeta^2}$ , or putting  $t^2 - c^2 = f^2$ , and  $t^2 - \kappa^2 = \phi^2$ , we have  $\kappa = \frac{t^2 c^2}{t^2 - f^2 \zeta^2}$ , and  $\phi^2 = \frac{t^2 \times 1 - \zeta^2}{f^2 - \zeta^2}$ .

### PROPOSITION III.

Fig. 3. Let BOD be an ellipsis, whose transverse diameter Ab makes the angle ACB, with the right line BCD, and let TKG be a tangent to the ellipsis, in the point F, making the angle GTC with the right line BCD: It is required to find the length of the normal Ck, drawn from the center of the ellipsis, to the tangent TG.

From C, the center of the ellipsis, let CE be drawn parallel to the tangent TG, meeting the ellipsis in the point E; and CG perpendicular to the line BCD, meeting the tangent in the point G: Put the sine of ACB = Z, its cosine =  $\dot{Z}$ , the sine of TGC = V, its cosine =  $\dot{V}$  (radius being unity) AC =  $t$ , CO =  $\kappa$ , and  $t^2 - \kappa^2 = \phi^2$ ; then will the sine of OCE (= the sine of OCD + DCE)

be expressed by  $\dot{Z}V + Z\dot{V}$ , and by the last proposition we shall find  $CE = \frac{t^2}{\sqrt{t^2 - \phi^2 \times \dot{Z}V + Z\dot{V}}}$ ; but (by conics)  $CE \times Ck = CO \times CA$ , whence we shall obtain  $Ck = \sqrt{t^2 - \phi^2 \times \dot{Z}V + Z\dot{V}}$ .

#### PROPOSITION IV.

Fig. 4. In the two similar right angled plane triangles HKS, HMN, right angled at K, and M, there is given the right lines KS and MS, to find the acute angles, supposing the given angle  $bnM$  to be nearly equal to the required angle HNM. Put  $MS = \Delta$ ,  $KS = r$ ,  $MN = v$ , the sine of the given angle  $bnM = q$ , its cofine =  $q'$ , the sine of HNM =  $V$ , its cofine =  $\dot{V}$ , the sine of  $HNM - bnM = x$ , and radius = 1. Let ML be drawn parallel to HK, and Ml parallel to SK: then in the right angled plane triangles NML, SML, we have as rad. (1) : MN ( $v$ ) :: sine HNM ( $V$ ) : Ml ( $vV$ ), and as rad. (1) : MS ( $\Delta$ ) : sin. LMS ( $\dot{V}$ ) : LS ( $\Delta\dot{V}$ ); but  $Ml + LS = KS$ ; therefore  $v\dot{V} + \Delta\dot{V} = r$ , and by the foregoing notation  $V = q + q'x$ , and  $\dot{V} = q' - qx$ ; therefore these values of  $V$  and  $\dot{V}$  being wrote in the above equation we shall find  $x = \frac{q\Delta + qv - r}{q\Delta - q'v}$ , and from thence  $V = \frac{\Delta - q'r}{q\Delta - q'v}$ , and  $\dot{V} = \frac{q' - v}{q\Delta - q'v}$ .

PROPOSITION V.

Fig. 5. If the opaque prolate spheroid  $BPOD$ , given in species and position, be opposed to the given luminous sphere  $HKQI$  at the given distance  $CS$ , forming the shadow  $Ffbc$ : It is proposed to determine the figure of the section  $aRN$  made by a plane, cutting the shadow perpendicularly to its axis at the given distance  $MS$ .

Let the required curve  $aRN$  be conceived to be generated by the extremity  $R$ , of the variable right line  $MR$ , revolving about the given point  $M$  as a center, the line  $MR$  being always perpendicular to the axis of the shadow  $MS$ : Let the right line  $RQ$  be a tangent to the sphere  $HKQI$  in the point  $Q$ , and in the same plane with the right lines  $RM$ ,  $MS$ , it will then represent one of the rays of light, which constitute the conical superficies of the shadow, and, therefore, by the laws of optics, will be a tangent to the spheroid also; now when the generating point  $R$  has arrived at  $N$ , the ray  $RQ$  (being supposed to revolve with it) will co-incide with the tangent  $NK$ , touching the sphere in  $K$ , and the spheroid in  $F$ : Join  $K$ ,  $S$ , and the angles  $NMS$ , and  $NKS$ , will be right angles; let the spheroid be supposed to be cut, by the quadrangular plane  $NMSK$ , forming thereby the elliptic section  $BOD$ , draw  $Ck$  perpendicular, and  $Cl$  parallel to  $NK$ ; put  $CA = t$ ,  $MC = \delta$ ,  $CS = \phi$ ,  $MS = \Delta$ ,  $SK = s$ ,  $MN = y$ ,  $CO = z$ , the sine of  $bNM = V$ , its cosine  $= \dot{V}$ , the sine of  $ACB = Z$ , its cosine  $= \dot{Z}$ , and radius  $= 1$ :  
Then

Then in the right angled plane triangle  $CL'S$ , it will be as rad. (1) :  $CS(d) :: \sin SCL(\dot{V}) : SL(d\dot{V})$ , and consequently  $Ck (= KS - SL) = r - d\dot{V}$ ; but by prop. III.  $Ck = \sqrt{r^2 - \phi^2 \times ZV + ZV^2}$ , whence we shall have  $r - d\dot{V} = \sqrt{r^2 - \phi^2 \times ZV + ZV^2}$ : Now by proposition I. we shall find  $\zeta = b' \times p + p'z$ ,  $Z = \frac{\beta}{\sqrt{\beta^2 + p' - pz}}^2$ , and  $Z' = \frac{p - pz}{\sqrt{\beta^2 + p' - pz}}^2$ ; by prop. II.  $x^2 = \frac{r^2 c^2}{r^2 - f^2 \zeta^2}$ , and  $\phi^2 = \frac{r^2 \times 1 - \zeta^2}{f^2 - \zeta^2}$ ; lastly by prop. IV.  $V = \frac{\Delta - q'r}{q\Delta - q'u}$ , and  $\dot{V} = \frac{qr - v}{q\Delta - q'u}$ , which values being substituted in the above equation will exhibit the nature of the required curve  $aRN$ , in terms of  $z$  and  $u$ .

## SCHOLIUM.

If the sphere  $HKQI$  represents the sun, and the spheroid  $BPOD$  one of the primary planets, it will appear, from the preceding reasoning, that the figure of the section of its shadow received upon a plane, which is perpendicular to its axis, will not be a circle (except when the axis of the planet produced passes through the sun's center) but a curve of the oval kind, whose species will be known from the foregoing equation.

If the sphere  $HKQI$  had been regarded as a spheroid in the above solution, it is easy to see that the foregoing process would have determined the nature

nature of the required curve; but the figure of the sun is so nearly spherical, that it was not thought necessary to embarrass the solution with that consideration.

Hence the duration of an eclipse of a given satelles may be determined in the following manner: Let  $BRC$  (fig. 6.) be the section of the shadow, through which the satelles passes,  $NpN$  the path of the satelles, making the given angle  $NpM$ , with the circle of latitude  $RpM$ ;  $BM C$  a part of the primary's orbit produced, and  $Mp$  the given latitude of the satelles at the time of the syzygia; the circle of latitude  $RpM$  is represented in fig. 1. by the primitive circle  $BEGD$ , and the angle  $RMN$ , by the spherical angle  $EBA$ ; therefore the  $\text{fine of } RMN = \text{the fine of } EBA = \text{the fine of } EBF + FBA + aBA = ap' + a'p + \frac{a'p' - ap}{\sin z}$ , and its cosine  $= a'p' - ap - \frac{a'p' + ap}{\sin z}$ ; which for the sake of brevity may be expressed by  $y$ , and  $y'$ ; then putting  $Mp = n$ ,  $MN = v$ , the  $\text{fine of } MpN = m$ , its cosine  $= m'$ , and radius  $= 1$ ; we shall have the  $\text{fine of } MNP$  expressed by  $my' + m'y$ ; and therefore we shall have in the plane triangle  $MpN$ , as the  $\text{fin. } MNp (my' + my) : Mp(n) :: \text{fine } MpN (m) : MN(v)$ ; hence  $v = \frac{m.n}{my' + ym'}$ ; from which, and the equation of the curve (determined above)  $\frac{v}{m} = pN$ , and consequently, the duration of the eclipse will become known.

In prop. I. the  $\text{fine of the angle } ABF$  is expressed by  $p + p'z$ , and its cosine by  $p' - pz$ , instead of their true values  $p'z' + pz$ , and  $p'z' - pz$ ; this was done

to render the following conclusions more simple than they otherwise would have been; and as the angle  $aBA$  is, by hypothesis, but small, its cosine will approach so near to the radius, as not to occasion any sensible error in the result; and the same may be observed with regard to what is advanced in prop. IV.

It remains now to apply, what has been investigated above, to the eclipses of Jupiter's satellites, and to examine whether the prolateness of his figure will have any sensible effect upon their durations; and this is become the more necessary, as that celebrated astronomer M. de la Lande (who candidly acknowledges, that he was excited to turn his thoughts upon this subject, from a cursory view of this paper, which was shewn him by Dr. Bevis\*) does not seem to have considered the question, with that degree of attention which I think it demands.

But before this can be done with exactness, it will be necessary to have the inclination of Jupiter's axis, with respect to his ecliptic, and the place of his equinoxes determined by observation, neither of which I believe has yet been done with any degree of certainty; I shall, therefore, proceed in this inquiry upon M. de la Lande's hypothesis, that Jupiter's axis is perpendicular to his orbit; and perhaps this supposition is not so far distant from the truth, as to occasion any material error in the conclusion. It may also be remarked, that in the general equation given above,  $\dot{V}$  and  $V$  express the sine and cosine of the semi-angle of the cone of Jupiter's shadow, but this angle can never exceed  $3'$ , and consequently we may very

\* Vid. *Connoiss. des Mouv. Celest.* 1765, p. 177.

safely use the radius instead of  $V$  wherever it occurs.

By this means the general equation will become  $r - d \dot{V} = \sqrt{r^2 - \phi^2}$ , or which is the same  $r - d \dot{V} = x$ , therefore  $\dot{V} = \frac{r-x}{d}$ ; but by prop. IV.  $\dot{V} = \frac{q r - v}{q \Delta - q' v}$ , which, because  $q$  is nearly equal to  $V$ , and  $q' v$  very small with respect to  $q \Delta$ , will become  $\dot{V} = \frac{r-v}{\Delta}$ ; therefore  $\frac{r-x}{d} = \frac{r-v}{\Delta}$ , from which we shall find  $v = \frac{\Delta x - \delta r}{d}$ ; and this equation is exactly the same with that which would arise from considering the sun as a circular, and Jupiter as an elliptic plane, limited by one of his meridians, and always parallel to the disk of the sun; which supposition, the immense distance of Jupiter from the sun renders very allowable.

From this equation an easy mechanical method may be derived of delineating the curve of the shadow, at any given distance from Jupiter, for as  $x$  denotes any semi-diameter of the elliptic section of Jupiter's body, it is manifest, that the term  $\frac{\Delta}{d} \times x$ , will express the corresponding semi-diameter of a similar ellipsis, whose axes are to those of Jupiter in the given ratio of  $\Delta$  to  $d$ , and the term  $\frac{\delta r}{d}$  is wholly given: Therefore if  $ar m$  (fig. 7.) be such an ellipsis, and there be drawn through its center  $M$  any number of semi-diameters  $Ma$ ,  $Mb$ ,  $Mc$ , &c. meeting the ellipsis



lipfis in  $a, b, c$ , &c. let  $a A, b B, c C$ , &c. be taken each equal to the given term  $\frac{\delta r}{d}$ , and the points  $A, B, C$ , &c. will be in the required curve.

It appears from considering the nature of this curve, that it will have two cusps, one at each extremity of its lesser axis, which will approach toward each other, according as the distance  $\delta$  is augmented; therefore, if the distance of the section of the shadow, from Jupiter's center, was taken, such that  $\delta = \frac{dc}{r-c}$ , the lesser axis of the curve would then vanish, and the cusps meet in the center, and thereby form two distinct shadows (as represented in fig. 8); in consequence of which, if a satelles, revolved at that distance, it might suffer a double eclipse, at the same conjunction, which remarkable phenomenon may also happen, at a less distance from Jupiter, in some circumstances.

I shall now shew how the duration of an eclipse of a given satelles may be determined independant of the equation of the curve; and this, perhaps, will be the more acceptable, as it will afford a practical rule, which may be applied, in every position of Jupiter's axis, with very little trouble. This may be done by the help of the following proposition.

## PROPOSITION VI.

If a circle  $eDfG$  be described about the conjugate axis  $GD$ , of a given ellipsis  $ADBG$ , and a right line  $EF$  be drawn, making the given angle  $F\pi D$ ,  
with

with the conjugate axis, and passing through the given point  $\pi$  taken therein, it is proposed to determine the length of the segments  $Ff$ ,  $Ee$ , intercepted between the circumference of the circle, and the perimeter of the ellipsis.

From the point  $F$ , draw the right line  $Fd$  parallel to the transverse axis  $AB$ , meeting the conjugate  $GD$  in the point  $d$ , and the circle in  $c$ ; draw the lines  $CF$ ,  $Cf$ ,  $Cc$ , and let  $c\pi$  be joined: Then by conics we shall have, as  $CB : CD :: \text{tang. } F\pi D : \text{tang. } c\pi D$ , and in the right lined triangle  $C\pi c$ , it will be as  $Cc (CD) : \text{fin. } C\pi c :: C\pi : \text{fin. } Cc\pi$ , whence the angle  $cC\pi$  becomes known; but as  $CD : CB :: \text{tang. } cC\pi : \text{tang. } FC\pi$ ; therefore  $FC\pi$  is known; from which taking away the given angle  $fC\pi$ , there remains the angle  $FCf$ ; consequently all the angles, in the right lined triangle  $fCF$ , together with the side  $Cf (CD)$ , are known: we shall therefore have, in the right lined triangle,  $FfC$ , as  $\text{fin. } fFC : Cf :: \text{fin. } fCF : fF$ , one of the required segments, and by a similar operation, the other segment  $Ee$  will be found, whence as  $ef$  is given,  $EF$  will become known.

### COROLLARY I.

The required segments  $Ff$ ,  $Ee$ , will be found in the same manner, when the given point  $\pi$  is not taken in one of the axes, but any where between; but in that case, the point where the line  $EF$  intersects the conjugate axis, must be first determined.

### COROLLARY II.

If a perpendicular  $Cn$  be let fall from  $C$  upon the line  $EF$ , the angle  $\pi Cn$  will be given, to which,  
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adding  $FC\pi$  (found above) the angle  $FCn$  will be known; hence we shall have the following analogy for determining  $Fn$ : As  $\text{tang. } /Cn : \text{tang. } FCn :: fn : Fn$ .

Now let  $KkH$  (fig. 10.) represent the disk of the sun, and  $eDfG$  that of Jupiter, considered as a circle, whose diameter is equal to his axis  $DG$ , draw  $Npn$ , the path of the satelles, making the given angle  $NpR$ , with a right light  $Rg$  drawn parallel to the diameter  $DG$ , and let  $ab$  be the duration of the eclipse, and  $V$  the apex of the shadow in this hypothesis; join  $Va$ ,  $Vb$ , and let the plane  $aVb$  be produced, till it meets the sun's disk in  $K$  and  $k$ , it will then intersect the disk of Jupiter in the line  $f\pi e$ , and the lines  $VK$ ,  $Vk$ , will also touch the circumference of the circle  $eDfG$ , in the points  $e$  and  $f$ , draw the line  $SV$ , and it will be the axis of the shadow, and consequently will pass through  $C$  and  $M$ , the centers of Jupiter, and the section of the shadow; join  $aM$ ,  $bM$ ,  $fC$ ,  $eC$ , and the triangles  $abM$ ,  $efC$ , will be similar to each other, and, therefore,  $abM$  being wholly given,  $efC$  will likewise be known. Let  $ADBG$  be the elliptic section of Jupiter's body, and produce  $e\pi f$ , both ways, till it meets the periphery of the ellipsis in the points  $E$  and  $F$ , draw  $KF$ ,  $kE$ , and produce them till they meet with  $ab$ , produced both ways in  $N$  and  $n$ , then will  $Nn$  be the required duration of the eclipse in the true shadow: Now the triangles  $KfF$ ,  $KaN$ , being similar, as are also the triangles  $keE$ ,  $kbn$ , and the segments  $Ff$ ,  $eE$ , being given by the preceding proposition, the required segments  $Na$ ,  $bn$ , will also become known, for they will be to the former segments in the given ratio of  $SM$  to  $SC$ .

It may be observed, that this method is equally applicable, whether the axis of Jupiter is perpendicular to his orbit, or not ; for if it is not, we can easily find by proposition I and II. the species and position of that elliptic section of Jupiter's body, to which a right line connecting the centers of the sun and Jupiter is perpendicular ; and this being obtained every thing else will remain as before.

As it would require more time, than I have to spare at present, to enter into a particular inquiry concerning the alterations, which this irregularity in the shadow will occasion, in the present theory of Jupiter's satellites, I shall conclude with observing, that the errors in the semi-durations of their eclipses, arising from this cause, may sometimes amount to  $20''$  in the first ;  $50''$  in the second ;  $2' 19''$  in the third ; and  $11' 14''$  in the fourth ; which errors will, I believe, be deemed sufficiently large to merit the attention of astronomers.

G. Witchell.

Received September 24, 1766.

IV. *An Attempt to account for the universal Deluge, by Edward King, Esq; of Lincoln's-Inn, F. R. S.*

Read Jan. 22.  
1767. **A**FTER so many conjectures as have been already formed concerning the cause of the universal deluge, it may perhaps appear both impertinent to attempt a new solution, and also useless, as theories formed on mere hypothesis are always uncertain, and little to be depended upon. But if we give them no more weight than they deserve, and, considering them only as small steps towards the investigation of truth, do not desire any further assent to our conclusions than the probability on which they are founded demands, even such kind of enquiries may be of service, and open a door to new discoveries.

Where we cannot arrive at demonstration we must be content with probability. Our despair of attaining the one ought not to make us neglect the other. And with regard to this remarkable event, the universal deluge, every degree of probability, even the smallest, that appears in an attempt to account for it philosophically, has its use; as it tends to remove those objections that are made to the truth of the fact, by persons who may not think the mere relation of it in the Mosaic writings a sufficient proof of the reality of it; or who may be led, from the difficulty there appears

appears in accounting for such an event, to doubt of the authority of those sacred books.

Many ingenious hypotheses have been already formed on this subject; but they all seem liable to most insuperable objections: and therefore I make no scruple to venture another into the world, which appears to me free from such difficulties as they are involved in, and more simple. I am willing, however, it should fall to the ground, as soon as there appear any reasonable and weighty objections to it. I only wish that the hints contained in this paper may be a means of leading some person of greater abilities to a more perfect discovery; and that it may always be remembered, that the fossil shells found in all parts of the earth, are a sufficient proof of the truth of its having been at some time or other entirely covered with water, however fallible any attempt to account for the deluge may be.

Dr. Burnet, in his theory, has given such an account of the deluge, as Dr. Keill has shown to be very improbable, and unphilosophical. He has first described the primæval earth so as to divest it of all beauty and elegance, and then has ascribed the deluge to such causes, as are not only somewhat inconsistent with that part of his theory, where he supposes the earth to be well watered and moistened with dew; but are also insufficient to account for the waters flowing over the tops of the mountains: since on the breaking of his imaginary shell, it is impossible to suppose that the waters of the abyss, even on such a concussion, should flow up high enough upon those parts that were left elevated, so as to cover the mountains that now subsist.

Mr.

Mr. Whiston has called in the assistance of another planetary body; and has supposed the tail of a comet to be so greatly condensed as to afford a quantity of water sufficient for this purpose. But, besides the inconsistency of this theory with that of gravitation, it is no less difficult, according to his hypothesis, to get rid of the water with which the earth was covered, than it is, according to others, to find a sufficient quantity.

Mr. Ray has accounted for this amazing event, by supposing a change to have happened in the center of gravity of the earth. But how to find a cause for such a change in the center of gravity, and for a restoration of it to the same place again, is more difficult, and the supposition of it more inconsistent with our philosophical ideas, than any other hypothesis whatever.

Such have been some of the principal theories hitherto advanced, and far be it from me to presume that mine may not in the end be found equally fallible; but it appears to me at present to be more plain and consistent, and at the same time is free from that great difficulty which has perplexed all the rest, and is indeed the most important difficulty in the enquiry, that is, the accounting for a sufficient quantity of water.

We find in the Mosaic history of the creation, that God at the first created sea as well as land; and therefore have grounds to believe both from thence, and from the reason of things, that there was as great a quantity of sea on the antediluvian earth, as there is now upon the earth in its present state.

We find also the whole surface of the earth to be undermined by subterraneous fires, which make their appearance

appearance in various places, in very formidable volcanoes. This has been the case in Italy, and amongst the Azores, in Tartary, in Kamtschatka, in South America, in Ireland, in the islands of the East Indies, and in other parts: and we have reason to believe that these subterraneous fires have made eruptions, not unfrequently, even in the bottom of the sea; as Mr. Mitchell has made appear in his excellent paper concerning the causes of earthquakes\*.

We have also, in the Philosophical Transactions, an account of entire islands being raised in the Archipelago, and likewise amongst the Azores, by such subterraneous fires †; and Mr. Ray, in his travels, mentions a mountain one hundred feet high, raised by the earthquake in 1538, which also threw up so much earth, stones, and ashes, as quite filled up the Lacus Lucrinus ‡.

To which may be added, that fossil shells and other marine bodies are so universally found in all parts of the present continents and islands, as to amount almost to a demonstration, that all the now dry land was once covered with sea, and that for a considerable space of time, probably much longer than the continuance of the deluge is related to have been. For though such a violent flux of waters might have thrown up some shells and marine bodies upon the hills and mountains, yet it could not have flung up such vast quantities, nor so universally. The prodigious beds of shells which we now find in all parts

\* Philos. Trans. Vol. LI. part II. p. 566.

† Philos. Trans. No 372, or Eames's Abr. vol. VI. part II. p. 203, and Jones's Abr. vol. V. part II. p. 196,

‡ Ray's Travels, old edition, p. 273.



cannot well be accounted for, but by supposing the waters, in which those shell-fish lived, to have covered the countries where they are now found, for a long time, and even for ages.

The supposition therefore, which I am about to advance, founded on these facts, is this; that originally Almighty God created this earth with sea and land nearly in the same proportion as they now remain, and that it continued in that state for many ages, during which the bottom of the sea became covered with shells, and various heterogeneous bodies; that from the first of its creation there were also many subterraneous fires found within the bowels of the earth; and that, at the appointed time, these fires bursting forth at once with great violence, under the sea \*, raised up the bottom of the ocean, so as to pour out the waters over the face of what was before dry land, which by that means became sea, and has perhaps continued so ever since, as that which was before the flood the bottom of the sea, probably from that time has continued to be continent and dry land †.

\* Mr. Mitchell has shewn, in his paper on the causes of earthquakes, that such subterraneous fires are at all times very liable to make eruptions under the sea, and that when they do so, the earthquakes consequent upon such eruptions are more extensive than any whatever.

† I do not mean by this to insinuate that all that part of the globe which is now sea was dry land before the flood; or that the antediluvian ocean was merely of the extent of our present continent. I apprehend, on the contrary, that there was always a greater proportion of water on the face of the earth than of continent; and I would only be understood to mean, that all that which was dry land before the flood is now buried under the sea, whilst that which was a part of the bottom of the antediluvian

This

This hypothesis may perhaps be liable to great objections; but it is at least consistent with what Moses relates of the fountains of the great deep being broken up; and, without any perplexity or difficulty, accounts at once for a sufficient quantity of water to cover the tops of the highest antediluvian mountains, even supposing they were left standing: though it is not improbable but that they might be thrown down by means of the same earthquake. If they were left standing, some of them might (on the retreat of the waters from their tops after the first concussion) form some of the islands that now subsist.

I must also add, that this hypothesis is perfectly consistent with, and perhaps in some measure accounts for, that singular position of the strata of coals, ores, and various kinds of earths (mentioned in Mr. Mitchell's paper), which are found always sloping from mountainous countries, and higher grounds, towards the bottom of the sea; so that what is nearest the surface of the earth, in mountains and high countries lies deepest in low lands and under the sea.

It is, besides, somewhat confirmed by that singular observation of Dr. Hasselquist's, in his travels, (p. 33) where, speaking of Natolia and the eastern countries in general, he says, "In no place was it more evident that the continent, we call earth, was in the beginning the bottom of the sea." Ulloa also informs us, that the same thing is evident in the whole country of Valles in South America \* : and Norden

ocean forms our present land: and that consequently some part of the ocean was sea both in the antediluvian earth and in the present state of it, and common to both.

\* Ulloa's voyage to South America, vol. II. p. 99.

tells us, that the rocks in Egypt bear evident marks of having been washed by the sea\*.

These are the reasons which induce me to venture upon this supposition; and now I will just consider one or two objections, that appear to me amongst the most material which may be made to what I have advanced.

It may perhaps be said, that we read † “ of the  
“ waters returning from off the earth, and of their  
“ being abated at the end of the hundred and fifty  
“ days: and also, of the waters decreasing continually  
“ till the tenth month; and of the tops of the  
“ mountains being then seen.” And it may be  
objected, that we ought from thence to conclude, that  
the waters of the deluge, having covered what was  
before dry ground, afterwards retreated, and left the  
very same hills and land dry again.

But this conclusion is by no means necessary; for  
all that can be inferred from what we find in Genesis  
concerning the decrease of the waters, is, that they  
gradually subsided from off the face of what is now  
continent and dry land, as of course they would do on  
the elevation of it, agreeable to the foregoing hypo-  
thesis. And indeed, if the deluge was effected in the  
way here supposed, we can then give a rational and  
easy account how all the water came to drain off the  
ground, and to leave it dry so soon as is recorded;  
which otherwise is a circumstance in this piece of  
history very perplexing. It is evident, that such a  
violent earthquake, or bursting forth of the subter-  
raneous fire, as is here supposed to have raised the

\* Norden's Travels, vol. II. p. 21.

† Genesis, ch. viii. 3—5.

bottom of the then sea (the present continents), at once as high or higher than what was before dry land, must in a very short time have drowned and overwhelmed the antediluvian earth, by pouring out the waters upon it; and it is also evident, that for some time the bottom of the sea, so raised, would continue covered with the waters, which, till the vast agitation into which they were flung subsided, would continue flowing backwards and forwards. But, by degrees, and very easily within the time mentioned in Scripture, the water would drain off from all the higher parts, and leave the new land quite dry, and in the state we now find it, with strata of shells, and sand, and stones, and other bodies, lying just as the sea had by accident many ages before placed them. Whereas, were the deluge occasioned only by an addition of water sufficient to raise the surface of the sea higher than the land and mountains, in that case, it is impossible to imagine any means, at all consistent with the course and laws of nature, by which such an immense body of water, could be evaporated or conveyed away in so short a space of time. And besides, in that case, the shells, &c. flung upon the land by the concussion of the waters, and subsiding there within so short a space of time, would rather be found lying according to their specific gravities: a fact which Dr. Woodward supposed certain, but which is by no means true. Nor indeed, according to the conjectures here advanced, is it at all necessary that it should be so. For, as I imagine the shells and other marine bodies, which are now found on various parts of the dry land, to have been placed there gradually during a succession of ages, whilst it was the bottom of the sea; it will

follow, that they must be found just as the sea, by its washings and motion, laid them; which would of course first wash many of them together, and then wash gravel, or sand, or clay, or other substances over them; after which, more shells or other bodies would be deposited, and then more stones or gravel, &c. according to the nature of the soil. In short, whatever was specifically heavier than water, would (after its removal by any agitation) soon subside, and remain fixed, whether the substances underneath it were specifically heavier than itself or no: it is sufficient that they were but all specifically heavier than the water.

We find to this day great changes are continually making, within the memory of man, both on the face of the earth, in the shores, and in the bottom of the sea, even in those small parts of it that we are acquainted with; and such changes must also have happened before the flood, and might very probably produce that situation of shells, &c. so different from what might be expected from their specific weights.

Another objection may perhaps be made by saying, if all the antediluvian earth was at once overwhelmed, and of course all its plants with it, whence came it to pass, that the ~~now day land~~ was so soon covered with vegetables and herbage of all kinds? To this I answer, in the first place, that the difficulty is just the same, whether we suppose the bottom of the antediluvian sea to be the present continents, or whether we suppose the face of the earth to have remained the very same; since, by the waters of the deluge, all plants, trees, and vegetables, must in both cases equally have been destroyed; and nothing could well remain,  
except

except some of their shoots and seeds; which might just as well take root on the new continent, on the subsiding of the waters, as on the old. And in the next place, I answer, that there are not a few instances (as is shown in Stillingfleet's tracts\*) of barren rocks and plains becoming by degrees well covered with verdure, though very remote from any places that might apparently furnish seeds. They have first borne a kind of moss, and afterwards other plants of an higher order (the seeds being brought there by accident, and by the various and admirable means of conveyance, which the Creator has given them), till at last they have been covered with rich verdure. To which may be added a very extraordinary fact, now well known, namely, that if a piece of ground which has ~~not~~ been cultivated be turned up, and the clods loosened, it will very soon produce a variety of plants, some of which were never known to grow there before. We find that one acorn is sufficient to produce a forest, and it is by no means to be supposed (let the deluge have happened how it would) that, immediately after it, ~~the earth was as well clothed with verdure, as it has become since.~~ Probably it was for a time in general very barren, except such parts as Noah and his sons cultivated, with seeds which they had preserved in the ark.

As to the leaf which the dove brought in †, that might be found on some plant which had taken fresh root immediately on the subsiding of the waters, or it

\* Stillingfleet's Tracts, p. 78, and also p. 45, where an instance is produced, much to the purpose, of ~~marshes~~ becoming by degrees fine meadows.

† Genesis, ch. viii. v. 11.

is not impossible but the top of some antediluvian mountain, having been but slightly covered, might on the ceasing of the first concussion (as I before observed) remain in the state of an island, elevated above the surface of the sea.

I apprehend, no objection of any weight can arise from the description of paradise in Scripture, nor from its being said that the ark rested on the mountains of Ararat: since, whether the continent was changed or no, there is no place now remaining that answers the description of the former; nor is there any thing said about the latter, that should lead us to conclude there ever was such a mountain as Ararat before the flood.

But, leaving these objections from the words of Scripture, and the history of the deluge; another may perhaps arise, from this circumstance, that shells are found in various parts of the earth, which are evidently not the shells peculiar to the seas adjoining, but such as belong to a different climate. This fact at first certainly seems to contradict what I have advanced: and yet, when well considered, it will perhaps rather be found to confirm my hypothesis. For let any one but look on a terrestrial globe, and he will instantly see, that the present continents are evidently not in the same climates as the present seas; and therefore, though the shells found in many places of the earth are not found in the neighbouring parts of the ocean; yet, when those parts of the earth were ocean, they might have had a very proper climate and situation there. Thus, for instance, we may observe that the Mediterranean is in a more southern climate than the neighbouring continent of Europe, and in a more northern climate than that of Africa. And the whole

whole continent of Asia is in a climate much more northern, than the neighbouring Indian ocean.

But, if this solution of the difficulty is not thought sufficient, it may be added, that so great a concussion, and such a change in the figure of the earth, as must have happened from the subterraneous fires elevating so many parts higher than they were before, might possibly affect the gravitation of the parts of the globe of the earth, and cause it to revolve round a different axis after the flood; whence there would undoubtedly arise a change of climate in all parts, sufficient to account for the present situation of shells, in places so foreign to the climates where shell fish of the same species are now found. And as I have before observed with regard to seeds, so it may also be observed with regard to shell fish, that the conveyance of a very few of each sort (by the flux of water) to the beds proper for them, would be sufficient to preserve all the various kinds, and to cause them now to be found in such numbers, in those parts of the ocean that are best adapted to each peculiar class.

Another thing proper to be taken notice of, is the horns and bones of terrestrial animals being found in the earth, together with fossil shells; which seems to contradict the supposition of the present continents having been originally the bottom of the sea. But with regard to this, I must beg leave to observe, that probably some of those bones have been deposited there since the flood, and have been covered by an addition of earth, as has happened also to some of the trees and woods that were cut down in this island by the Romans. And, as to the rest, it cannot  
be



be supposed, but that on the first great eruption, which poured the waters of the ocean upon the dry land, there must have been a violent agitation for some time, by their flowing backward and forward; during which interval, the bodies of many terrestrial animals (floating on the water) would be washed to different parts of the new-raised continent, and be left there as the water subsided.

Some little objection perhaps may arise, from its being observed, that the sea at present covers a much greater part of the globe than the dry land does.

But I apprehend this was also the case before the flood; and it may easily be conceived, that some part of the bottom of the antediluvian ocean might be flung in the manner supposed in this paper, and not the whole; and that the bottom of the present ocean consists not only of what was before the flood dry land, but also of some part of what was, even from the beginning, the bottom of the sea.

I will therefore only just add, that probably the same subterraneous fires (which originally raised the continents and islands that now appear, and have ever since been making great changes in the bowels of the earth, and producing those tremendous earthquakes, which have happened from time to time) may in the end break forth with redoubled violence, and destroy it, in the manner foretold in Scripture.

It may not be amiss to add, in confirmation of the foregoing hypothesis, that the beds of shells, discovered in chalk pits, gravel pits, and other places, consist generally of one or two, or at most of a very few

few different sorts in each particular place, as they would of course do upon a supposition that those respective beds were formerly at the bottom of the sea, in the several places where those different kind of shell fish lived and bred ; and that they were from thence, together with the bottom of the antediluvian ocean, raised up by the force of subterranean fires : for we may observe in the present seas, that one species of shell fish take up their habitation in one place, whilst those of a different species are found in some other ; and that numbers of the same kind, as for instance cockles, or oysters, are generally found on the same banks. The present appearance of fossil shells, therefore, does at least in this respect seem consistent with the conjectures here advanced : whereas, upon a supposition that these fossil shells were carried to their respective places, at the time of the flood, merely by the torrent of water that then flowed to and fro, they ought rather to be found mixed promiscuously together ; and not those of one species in one place ; and those of a different species in another. And I beg leave here to mention, that, since the writing of the foregoing paper, I find an hypothesis somewhat similar to what is here advanced was adopted by Lazzaro Moro, a Venetian author, who asserts that the continents were originally raised by subterranean fires ; but he considers this merely as the cause of their first and original formation, and not as having occasioned the deluge, nor as having happened at that time.

Received September 24, 1766.

V. *An Attempt to account for the formation of Spars and Crystals.* By Edward King, Esquire, of Lincoln's Inn, F. R. S.

Read Jan. 29, 1767. **I**N all our enquiries concerning the works of the Supreme Being, and in our endeavours to investigate the secondary causes by which the various phænomena of nature are effected, we certainly cannot be too cautious and circumspect. Our comprehension of things is at best very imperfect; and we may easily be led, by too hasty a view, to draw false conclusions. It therefore becomes us to be very modest, and to endeavour to go upon sure grounds, before we indulge any hypothesis whatever. But if we observe this rule, we cannot carry our searches or conjectures too far, since the result of them will often be the discovery of what may be useful to mankind, and will almost always increase our knowledge in some degree, and be a means of raising in our minds more noble and exalted ideas of the Almighty Creator of all things.

This consideration encourages me to fling together the following conjectures, with regard to the production of those beautiful and transparent substances, which go by the name of spars, crystals, and jewels; but I would not venture to consider them as amounting to any thing more than queries.

I was

I was first led to entertain these thoughts, by observing the nature of Bristol stones, and the various states in which they appear; and was encouraged to think they might have some little weight, by observing also the nature of spars, and of those stony concretions found in large caverns. I shall, therefore, first mention the observations I made, and then draw my conclusions.

1. And in the first place, it is known, that the Bristol stones grow within the hollow cavity of some other rough stone; and that the substance of the external stone is porous, and frequently so strongly impregnated with crystalline corpuscles, that they glitter amongst the earthy particles, when held up to the light.

2. In the next place, it is to be observed, that, wherever there is a hollow cavity in these kind of stones, the inside is almost always lined with such shining substances, either in a perfect or imperfect state.

3. We find the Bristol stones appear in several different states; for in some places of the cavity, where the crystallization is not completed, they are of a dusky red, without any transparency; in others they appear of a dirty yellow; and in others white; and at last transparent.

4. As to the spars and crystals formed even in flints, and other hard bodies; I think they are generally observed in such as have evidently been at one time or other in a soft state, and lay in or near moist places strongly impregnated with saline particles of some kind or other; or else they are found in bodies wherein some saline and moist substances have formerly

merly been inclosed, and prevented from evaporating ; of which kind are the spars found in fossil shells, wherein the bodies of the shell fish have perhaps lain and perished.

5. We observe, not only in the small cavities of stones, but also in large caverns, such as those in the Peak in Derbyshire, Okeyhole in Somersetshire, and the famous grotto in the Greek island of Antiparos, and in short wherever moisture descends through the earth to a void space, and stops upon the inward surface, that it there forms crystals, or spars, or stony concretions of some sort or other ; of which some are so very imperfect, as to have only the appearance of rude heaps of petrified matter, without any regular form, which chiefly happens where there is much moisture, and where it descends, or soaks through pores so large as to carry many earthy particles with it.

6. To all which I must add, that Sir Isaac Newton has made it appear, that the transparency of bodies is occasioned by the minuteness of their pores, and the opacity of them by the largeness of the pores, in which the rays of light being reflected from side to side are lost, and prevented from passing through ; whence it is, that paper becomes transparent by being oiled, and the oculus mundi stone by being soaked in water.

These are the principal observations on which I found my conjectures ; and from hence I am induced to conclude, that all these above-mentioned substances, are formed by means of those crystalline (perhaps saline) corpuscles with which the surrounding earth or porous stones abound, and which probably are  
diffused

diffused throughout the whole globe, and mixed in some degree with most stratas. These small particles, I apprehend, are carried along gradually, by the moisture, or vapors, which soak through the pores, till they come to some cavity, and there, being stopped by the discontinuance of the earthy or stony substance from proceeding any further, they collect together in drops, and as they dry and harden, do of course, by their mutual attraction, form themselves into crystalline figures; and as the pores are more and more filled up, by the accession of more corpuscles, or by their mutual attraction which draws them closer together, they become more and more transparent. Some, however, of the bodies thus formed never have any transparency at all, being mixed with too many earthy or stony particles, or other heterogeneous matter, and have sometimes so much of that as not to be able to put on any regular form, but only to petrify in a confused heap; the earthy or stony particles preventing the crystalline or saline particles from forming themselves, by their mutual attraction, into regular figures; and there being perhaps but few of the true crystalline corpuscles mixed with them. This seems to be the case with many of the stony concretions in large caverns: and perhaps, from a small mixture of these same heterogeneous particles it is, that spars are inferior to crystals, and also differ from one another. Mr. Platt, in the Philosophical Transactions, Vol. LIV. p. 41. has observed, that spar seems to be nothing but crystal debased by a calcareous earth.

I cannot help suspecting, that what I have called crystalline corpuscles are in reality a kind of salts; I will beg leave, therefore, to call them hereafter by

that

that name ; and will just endeavour to illustrate what I have said more particularly by the instance of Bristol stones. In their first state, these are of a dirty red, or some other dusky color ; but afterwards, as more salts, or crystalline corpuscles, are added, by the descent of moisture, or the passage of more vapor, they begin to be more compact ; and then, the pores becoming smaller, they approach nearer to transparency, and put on a yellow or whitish color ; and at last, receiving a further addition of salts, and having the component particles drawn still closer together by their mutual attraction, they become still harder and more transparent, till they acquire, by a length of years, their greatest degree of perfection.

As to the Bristol stones being found of such different sizes ; I am induced to think they grow larger or continue small, only by the accident of the moisture bringing salts to them faster or slower ; for had they any other regular method of growing, I think they ought never to be found in their most perfect state, till they had first acquired their full bulk ; whereas, on the contrary, they are found in their greatest degree of perfection of all sizes.

I therefore imagine, that, till the outside surface is hardened, whatever addition of salts is made will increase the bulk of the stone ; but that, after the outward surface is once hardened, the addition of salts then only helps to bring it to its most perfect transparent state ; and therefore, that when the flow of moisture or vapor, and consequently the addition of salts, is very quick, there (the outward surfaces not having time to harden till a considerable quantity of salts are accumulated together) the stone, will grow large ;

large; but where the flow of moisture is slow, there (the outside surfaces, and indeed the whole mass, becoming hard before a fresh supply of salts is added) the stones will be small. And again, where the moisture and salts pass through large pores, there the crystal, or spar, or other concretion, will be very imperfect, being mixed with much heterogeneous matter; but where the moisture, and crystallizing particles, or salts, descend through very minute pores, there the salts will be most unmixed and pure, and the crystal or stone will be of the most transparent kind. In short, I cannot but look upon the largest caverns in the earth, and the smallest cavities in stone, as producing similar effects, and therefore consider them in the same light.

In these conclusions I may perhaps be mistaken, but they are at least consistent with the observations we make: for wherever there are cavities in the earth, or in stones, into which moisture can any way descend, we almost always find these kind of crystallizations and concretions; and the more plentiful the moisture is, and the more porous the strata of earth or stones are through which it passes, the larger the concretions are, and the more remote from a transparent state; as appears in those great caverns in the Peak, and in Somersetshire, &c. Whereas, on the contrary, the harder and less porous the substance is through which the moisture passes, the more transparent are the stones formed by it, as in the case of Bristol stones, and of some of those beautiful spars adjoining to veins of ore.

Whether all kind of stones may not be formed in somewhat the same manner, by the water carrying the  
the



the stony particles to the same place, and their collecting there together, by their mutual attraction, I leave to others to determine; but I am much persuaded, that this may probably be the manner of the production of spars and crystals: and perhaps jewels, or precious stones, may grow just in the same way; and owe their perfection solely to their being composed of still more minute salts, and more slowly; whence we may conjecture, why it is so rare to find large diamonds. I have observed some of the Bristol stones to have a fine purple appearance, like an amethyst; and it is well known, that several sorts of spars are of various beautiful colors, by means of a mixture of mineral particles, in which they have a distant resemblance of jewels; and indeed they seem to be very analogous to them in many respects.

After all, however, I am sensible that what I have advanced deserves not to be considered as any thing more than mere guesses. I know that a confirmation of the truth of these conjectures must depend upon experiments, which I have it not in my power to make: and I can only conclude with wishing, that some gentlemen, conversant in chemistry, may some time or other attempt to analyze these bodies, and to see whether they really are composed of what may properly be called salts, and of what kind.

It is much to be lamented, that, in enquiries into the nature of fossils, there have not been more chemical experiments made.

Received October 10, 1766.

VI. *Experiments with Camphire, by Mr. Alexander, Surgeon in Edinburgh.*

Read Jan. 29,  
1767.

**A**S medical authors have differed so widely in their opinions concerning the nature and effects of camphire, one part of them positively affirming that it heats, and another asserting with the same confidence that it cools the body; I made the following experiments with it, in order, if possible, to have cleared up the difficulty.

If camphire was a heater, I concluded it would raise my pulse, and augment my natural heat; and therefore, previous to my taking it, I counted the number of pulsations in a minute, which were sixty-eight, and found that, in the space of five minutes, the mercury in Fahrenheit's thermometer arose eighteen degrees by the heat of my stomach \*.

Having thus found the state of my pulse, and of my natural heat; I took  $\frac{1}{2}$  of camphire in a little of the pulp of tamarinds; and twenty minutes afterwards applied the thermometer to my stomach: the mercury, in the space of five minutes, arose exactly eighteen degrees, as it had done before taking the dose, but my pulse beat only sixty-six, which was two strokes less.

Three quarters of an hour after I had taken the camphire, I applied the thermometer again; in the

\* The thermometer was applied to the pit of my stomach.

same space of time, the mercury arose exactly the same as in the last trial, but my pulse beat only sixty-five which was one stroke less, and three fewer than it had done before I took the camphire.

The next day, having found that the mercury arose nineteen degrees in five minutes, by the heat of my stomach; and that my pulse beat seventy-seven in a minute, I took  $\frac{1}{2}$ ij of camphire in a little of the syrup of pale roses: immediately after swallowing it, I felt a sensation in my mouth something similar to that occasioned by strong peppermint-water, but much more disagreeable; ten minutes after I had taken it I applied the thermometer to my stomach; in five minutes the mercury arose eighteen degrees, which was one degree less than it had done before I had taken it. My pulse now beat only seventy, whereas before I took the dose it had beat seventy-seven: five and twenty minutes after I had taken it, I applied the thermometer again, and the mercury arose the same as at the last trial, but my pulse had increased from seventy to seventy-seven, the exact number which it had beat before I took the camphire: soon after this, my head grew so very giddy that it was with great difficulty I could walk through the room. In this condition, I had an inclination to breathe the fresh air, opened the window and looked over into the street, where every thing appeared to me in the utmost tumult and confusion; feeling myself in danger of tumbling from my station, I shut the window and staggered from it to bed, threw myself down upon it, and having a book with me, endeavoured to read, but had no distinct idea of any one sentence, and far less could I connect two or more of them together so as to comprehend the meaning of the author:

author : not being able to amuse myself by reading, I arose, to see whether I could walk any better, but, to my great mortification, found that I was more giddy and could hardly walk at all. I then returned to the bed, and feeling myself thirsty, called for some mutton broth ; it being dinner time, the servant, instead of bringing the broth, covered the table as usual, not knowing that I was any way disordered. Seeing the dinner on the table, I got out of bed again, and with no small reluctance, swallowed down a plate-ful of the broth, but could neither taste bread nor meat, on account of a nausea, which however was not accompanied with any inclination to vomit.

I now staggered again to the bed, and took up the book I had left there, with a design to divert the attention of my mind into some other channel than that into which the present confusion of my ideas had hurried me : at this time, self-preservation suggested to me the expediency of taking a vomit ; but as I felt very little pain, and was not apprehensive of much danger, I resolved not to spoil the success of my experiment by evacuating the camphire before I should discover what its effects would be. Hitherto, amid a tumult of indigested ideas, I had retained some degree of sensibility ; but now there arose such a noise in my ears, the confusion and giddiness of my head increased so much, that all consciousness of what was present, as well as memory of what was past, were soon entirely obliterated, so that whether I endeavoured to read in the book I had taken up, or what else I did, I know not.

Fortunately, at this juncture, one of my young gentlemen came into the room, who told me, after I recovered,

recovered, that I desired him to shut the windows, and threw myself backward on the bed, where I lay a few minutes very quiet, after which, in a sort of frenzy, I started up and sat upon the side of it, made some efforts to vomit, but evacuated nothing; that I then threw myself back again, fell into strong convulsions, foamed at the mouth, shrieked with great violence, stared dreadfully at, and endeavoured to grasp and tear every thing around me. This outrageous fit was succeeded by a calm, something similar to fainting, during which time a relation was sent for, who came between three and four o'clock; when he spoke to me, I awaked, as I thought, from sleep, and knew him, though almost intirely insensible to every other object. Soon after, came Dr. Cullen, who had been sent for also; when he had felt my pulse, which beat one hundred in a minute, he ordered me to be blooded; but as it is probable that natural antipathies will remain when every other sensation is nearly lost, I obstinately refused to undergo this operation, on account of an unsuperable aversion I have to it. All this time, no person knew any thing of my having taken the camphire, nor did I recollect any thing of it myself; and though I was recovered so much from the fit I have just now described, as to know every one about me, I neither knew where I was, nor what I did.

As I felt a very uncommon sensation of heat, I got violently out of the bed, and threw myself on the floor, the coolness of which was very agreeable to me; upon which some cold water was brought, and my hands and face bathed in it; this proved still more agreeable, and in some degree quieted a tremor which had seized on every part of my body. At this time,  
Dr.

Dr. Monro, junior, who had also been sent for, came to my assistance. As I could give him no account of the cause of my illness; while he was walking through the room, he accidentally cast his eyes on a paper I had left on the table, containing the relation of my having taken the camphire, and its effects upon me, so long as I had been able to mark them. Upon this discovery, he immediately ordered me warm water; of which having drank pretty plentifully, I soon vomited, and, though more than three hours had passed, since I had taken the camphire, a great deal of it was evacuated in an undissolved state.

While I was holding my head over the basin into which I was vomiting, the smell of the camphire arose very strong, and first made me recollect that I had taken it, though I could give no distinct account of the time when, or manner how. I now, by the Doctor's order, drank the juice of two or three lemons and oranges, but was not sensible of any benefit from them. I mentioned before, that I had not only lost all remembrance of my past actions, but also the knowledge of almost every present object; but I now began, in some degree, to recover both; though in a manner so extraordinary, as I cannot possibly describe, so as to give a clear idea of it. Among the first things I recollected, was, that I had that day visited several patients; but I could neither discover their diseases, names, number, nor any other circumstance relating to them. I could likewise recollect, that I had formerly known a great many things, of which I was become intirely ignorant, but could not fall on any method of recovering that knowledge which I had lost. A person

son who has lost his senses by liquor, as soon as he recovers, is perfectly well acquainted with every thing he knew before : but the case was very different with me, for the furniture of my room, and almost every other object on which I cast my eyes, appeared as strange, and new to me, as if I had only that moment begun my existence ; and though I could remember the name of any thing when I looked at it, yet it was not without investigating its nature, that I could discover its use.

I had been put to bed when I vomited, and I know not whether it was owing to it, or the camphire, but I had now a severe head-ach, which disturbed me not a little all the evening. Between five and six o'clock I arose, and drank a bowl of tea, and the diluted juice of some more lemons and oranges. The giddiness in my head, ringing in my ears, excessive heat and tremor, which I had felt so severely before, were now considerably abated, though far from being intirely gone off. About seven o'clock, I had another visit from Dr. Monro, who, upon numbering my pulsations, found they were now reduced from one hundred to eighty ; in a minute after this, the thermometer was applied to my stomach, and in half an hour the mercury arose two degrees above blood-warm ; it was then removed from my stomach to the Doctor's, and the mercury fell more than one degree.

Between eight and nine o'clock, though I was considerably better, I still felt an uneasiness of body, and a confusion of mind, which it is impossible to describe ; on account of which, I went to bed, and very soon fell into a calm and soft repose, which continued,

continued, without any interruption, till next morning. When I awaked, I found my head-ach quite gone, though a small degree of the confusion in it still remained. Upon going to stool that morning, I was extremely costive, though I had not been so before, nor continued to be so after. All that day I felt a great soreness, and rigidity over my whole body, as if I had caught cold, or undergone some severe exercise; the next day I was something better, and the day following quite recovered.

As the foregoing experiments had not fully satisfied me, whether camphire acted as a heater or cooler on the body, I resolved to try if it would give any additional heat or cold to fluids, in which it was dissolved; but, after repeated trials, I found that it never altered the natural heat of spirits, or oils, in whatever degree they were impregnated with it.

The first dose I took was a moderate one, and appeared to have acted as a cooler; but the next, if there is any trusting to the sensations occasioned by it, or to the increased celerity of the blood, certainly must have heated to a very great degree.



VII. *A Description of a very remarkable aquatick Insect, found in a Ditch of standing Water near Norwich, in the Spring of the Year, 1762. By Edward King, Esquire, of Lincoln's-Inn, F. R. S.*

Read Feb. 5,  
1767.

**T** A B. IV. A is the female, and B the male, both represented on their backs, in the posture in which they usually swim; *a, a,* are a number of small transparent, fringed, fins, placed parallel, and contiguous to each other. They are almost always in a waving motion, and the animalcules seem to keep themselves suspended, at different heights, in the water, by means of them; for on their ceasing to move they sink to the bottom: *d* is one of those fins belonging to the female seen in front, and *b* is one of those belonging to the male, in which there is a very remarkable difference: *c* is the head of the female; and *g* is the head of the male; distinguished by three projecting substances like horns, or tusks which are marked *k* in figure B; one of the long ones on the side is drawn separate at *e*, and the crooked one in the middle at *f*; this last probably serves as a kind of trunk, and the former may be of service to catch their prey, whatever it is; *i* is a very singular projecting substance in the male, and may, perhaps, contain the parts of generation; and *b* is the ovarium of the

the

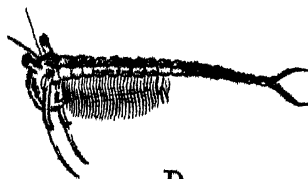
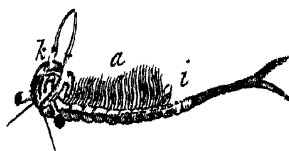




A



B



D

C



the female, in which (it being quite transparent) the ova, or spawn, are very visible, and may be seen, from time to time, to change their places, and to have a kind of circulation.

C is a view of the female, placed on its back, in order to shew the position of the fins, and their appearance when one looks down upon the insect; and D is the male, placed with his back uppermost, in the posture in which he sometimes lies still at the bottom of the water. Lastly, *b* is the tail magnified in a microscope, showing the hairs which grow on both sides out of it; but as the animalcule did not lie still long enough in the water, I could not view it with a glass so exactly as I wished to do, and, therefore, am not sure of the accuracy of the drawing of this part; all the other parts I have drawn as carefully as I was able, and they are about the natural size.

In these insects, besides their form, several particulars are very remarkable. 1. Their bodies are entirely transparent, and mostly of a yellowish hue, except towards the tail, and part of the ovarium, where the color is reddish; and, through a long vessel, which reaches almost the whole way, from the head to the tail, somewhat of a circulation, by fits and starts, is very visible, even to the naked eye. 2. In the ovarium of the female, the ova (which are of a mixed color in different parts, some brown, some yellow, and some red) are also in a constant circular motion round the bag (or at least, by a deception of sight, they appear to be so). 3. They swim constantly on their backs, keeping themselves suspended by the vibrations of their numerous fins, and moving forwards, by giving a sudden spring with  
 Vol. LVII. L their

their tails; which latter circumstance is common to almost all aquatick insects.

In the ditch from whence these were taken, there were a vast multitude of the same kind, though they have not been found in any other place that I know of. From their being prolifick in this state, I suspect it to be their only one, and that they are merely aquatick, and never turn to flies, as many insects found in water do: but then it seems very unaccountable, how they came to be in such abundance in this ditch, and no where else, at least so as to be observed.

They were discovered by a poor man, now dead, whose genius was very extraordinary, and much superior to what is usually found in his rank of life. He was indefatigable in his searches after every thing curious in nature, and, without ever having had any advantages of education, had acquired a degree of knowledge by no means contemptible. He kept a great number of these insects for a considerable time, and they seemed to receive their nourishment chiefly from the water itself, or from the most minute animalcules in it; not being perceived to feed on any thing that could be taken notice of. I had several of them for some days in my custody, from which I took this drawing; and they were seen and observed by many persons.

VIII. *An Account of the very tall Men, seen near the Streights of Magellan, in the Year 1764, by the Equipage of the Dolphin Man of War, under the Command of the Hon. Commodore Byron; in a Letter from Mr. Charles Clarke, Officer on board the said Ship, to M. Maty, M. D. Sec. R. S.*

Weathersfield, November 3, 1766.

S I R,

Read Feb. 12, 1767. **I** Had the pleasure of seeing my friend Mr. M—— a few days ago, when he made me acquainted with your desire of a particular account of the Patagonians, which I most readily undertake to give, as it will make me extremely happy if I can render it in the least amusing or agreeable to you. I wish I could embellish it with language more worthy your perusal; however, I will give it the embellishment of truth, and rely on your goodness to excuse a tar's dialect.

We had not got above ten or twelve leagues into the streights of Magellan, from the Atlantic ocean, before we saw several people, some on horseback and some on foot, upon the north shore (continent), and with the help of our glasses could perceive them beckoning to us to come on shore, and at the same time observed to each other that they seemed of an



extraordinary size; however we continued to stand on, and should have passed without taking the least farther notice of them, could we have proceeded, but our breeze dying away, and the tide making against us, we were obliged to anchor, when the commodore ordered his boat of twelve oars and another of six to be hoisted out, manned and armed. In the first went the commodore, in the other Mr. Cummings our first lieutenant and myself. At our first leaving the ship, their number did not exceed forty; but as we approached the shore, we perceived them pouring down from all quarters, some galloping, others running, all making use of their utmost expedition. They collected themselves in a body, just at the place we steered for. When we had got within twelve or fourteen yards of the beach, we found it a disagreeable flat shore with very large stones, which we apprehended would injure the boats; so looked at two or three different places, to find the most convenient for landing. They supposed we deferred coming on shore, through apprehensions of danger from them, upon which they all threw open the skins which were over their shoulders, which was the only cloathing they had, and consequently the only thing they could secret any kind of arms with, and many of them laid down close to the water's edge. The commodore made a motion for them to go a little way from the water, that we might have room to land, which they immediately complied with, and withdrew thirty or forty yards; we then landed, and formed each man with his musquet, in case any violence should be offered. As soon as we were formed, the commodore went from us to them, then at about twenty yards distance;

distance; they seemed vastly happy at his going among them, immediately gathered round him, and made a rude kind of noise, which I believe was their method of singing, as their countenances bespoke it a species of jollity. The commodore then made a motion to them to sit down, which they did in a circle with him in the middle, when Mr. Byron took some beads and ribbons, which he had brought for that purpose, and tied about the women's necks, &c. with which they seemed infinitely pleased. We were struck with the greatest astonishment at the sight of people of such a gigantic stature, notwithstanding our previous notice with our glasses from the ship; their number was increased by the time we got on shore to about five hundred, men, women, and children. The men and women both rid in the same manner; the women had a kind of belt to close their skin round the waist, which the men had not, as theirs were only flung over their shoulders, and tied with two little slips (cut from the skin) round the neck. At the time of the commodore's motion for them to retire farther up the beach, they all dismounted, and turned their horses loose, which were gentle and stood very quietly. The commodore, having disposed of all his presents and satisfied his curiosity, thought proper to retire, but they were vastly anxious to have him go up into the country to eat with them; (that they wanted him to go with them to eat, we could very well understand by their motion, but their language was wholly unintelligible to us.) There was a very great smoke to which they pointed, about a mile from us, where there must have been several fires; but some intervening hills prevented our seeing any thing but  
the

the smoke. The commodore returned the compliment, by inviting them on board the ship, but they would not favour him with their company, so we embarked and returned to the ship. We were with them near two hours at noon day, within a very few yards, though none had the honour of shaking hands but Mr. Byron and Mr. Cummings; however, we were near enough and long enough with them to convince our senses so far as not to be caviled out of the very existence of those senses at that time, which some of our countrymen and friends would absolutely attempt to do. They are of a copper colour, with long black hair, and some of them are certainly nine feet if they don't exceed it. The commodore, who is very near six foot, could but just reach the top of one of their heads, which he attempted, on tip toes, and there were several taller than him on whom the experiment was tried. They are prodigious stout, and as well and proportionally made as ever I saw people in my life. That they have some kind of arms among them is, I think, indisputable, from their taking methods to convince us they had none at that time about them. The women, I think, bear much the same proportion to the men as our Europeans do; there was hardly a man there less than eight feet, most of them considerably more; the women, I believe, run from  $7\frac{1}{2}$  to 8. Their horses were stout and bony, but not remarkably tall; they are in my opinion from 15 to  $15\frac{1}{2}$  hands. They had a great number of dogs, about the size of a middling pointer, with a fox noise. They continued upon the beach till we got under way, which was two hours after we got on board; I believe, they had some expectations

tions of our returning again ; but as soon as they saw us getting off, they betook themselves to the country.

The country of Patagonia is rather hilly, though not remarkably so. You have here and there a ridge of hills, but no very high ones. We lay some time at Port Desire, which is not a great way to the northward of the streights, where we traversed the country many miles round ; we found firebrands in different places, which convinced us there had been people, and we suppose them to have been the Patagonians. The soil is sandy, produces nothing but a coarse harsh grass, and a few small shrubs, of which Sir John Narborough remarked, he could not find one of size enough to make the helve of a hatchet, which observation we found very just. It was some time in December we made this visit to our gigantic friends. I am debarred being so particular as I could wish, from the loss of my journals, which were demanded by their lordships of the admiralty, immediately upon our return ; but if any article is omitted which you are desirous of being acquainted with, I beg you will take some means of letting me know it, for I will most readily communicate every circumstance of the matter, that fell under my observation, as it is with the greatest pleasure and respect that I subscribe myself,

S I R,

Your very humble servant,

Charles Clarke.

IX. *A Letter from Mr. William Sharp, Surgeon to St. Bartholomew's Hospital, to James Parsons, M. D. F. R. S. containing an Account of a new-invented Instrument for fractured Legs.*

S I R,

Read Feb. 12, 1767. **A**S the following treatment of fractured legs (from the experience I have had of its success during a practice of several years) appears to me preferable to any I have hitherto known, and as it may be a means of lessening many of the inconveniences attending such accidents, I take the liberty of sending it for your opinion; and, if you think it of consequence enough to be made public, shall be glad to have it laid before the Royal Society.

The instrument here recommended was first applied with great success in an oblique fracture of the tibia (which could not be kept in a proper situation by the usual methods), and afterwards, as happily, in a dislocation of the lower extremity of the same bone, accompanied with a fracture of the fibula. In this latter case, it is often difficult to reduce the dislocation even with a strong extension, and more so to retain the bones in their proper situation, while the limb is laid in the usual extended posture. But both these difficulties are absolutely avoided by the means I am about to describe.

The

The remarkable good effects in the cases above-mentioned, induced me to try the same in different fractures of the leg, as well compound as simple; in all which I have found it to answer my expectation.

I have formerly explained this method of treating fractures to many gentlemen of the profession, as well as to yourself; and have also had several cases where other surgeons have been concerned with me, who have in general expressed great satisfaction (and some of them have introduced it into their own practice); so that I have reason to believe it would have become more general, if the instruments, that have hitherto been sold for that purpose had been made according to the original pattern; but the workman, whom I employed, has made and sold many that differ from mine in some essential points. I have therefore thought it necessary to send you, inclosed, a description of the instrument, made after such a manner as I have found by experience to succeed best.

. I am, with great esteem,

S I R,

Your most obedient, and

most humble servant,

Mincing-Lane,  
Nov. 10, 1766.

W. Sharp.

*Description of a new invented Instrument for fractured Legs, (consisting of two Parts, which at present I shall call upper and under splints) recommended to be used instead of the common Apparatus. (See the Plate.)*

THE figures are drawn on a scale of three inches to a foot, and represent two splints of strong pasteboard, made with glow, to be fastened upon a fractured leg, by three straps which surround the whole.

These are adapted to the leg of a middle-sized man; nevertheless it may be convenient to have two other sizes, the one about twenty two inches in length, and the other sixteen.

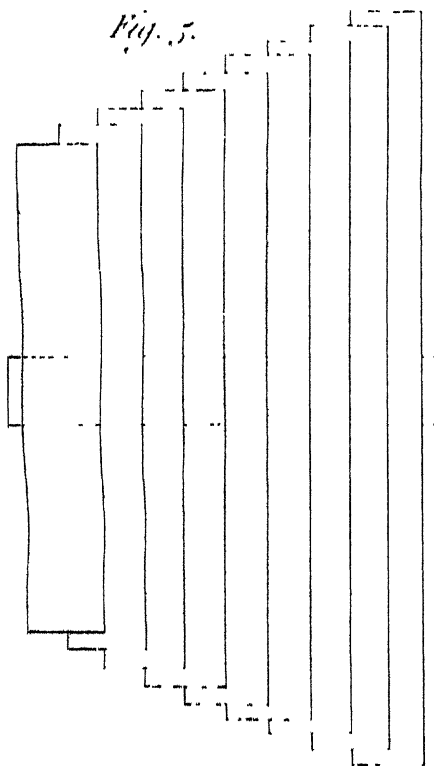
TAB. V. Fig. I. A represents an under splint of an irregular form, suitable to that part of the leg it is intended to cover; it is a little convex externally, and concave internally. The length eighteen inches, from *a* to *b*. The width two inches and three quarters, at the strap near the knee, and two inches and a quarter at both the other straps.

BBB. Three leather straps from fifteen to twenty inches long, and one inch wid., having two rows of holes so placed that every hole in each row may be opposite to a space in the other. These must be sewed fast to the middle and outside of the under splint. The portions of straps *ddd* on the anterior

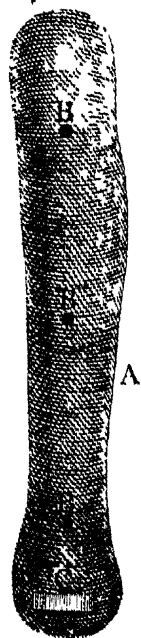




*Fig. 5.*



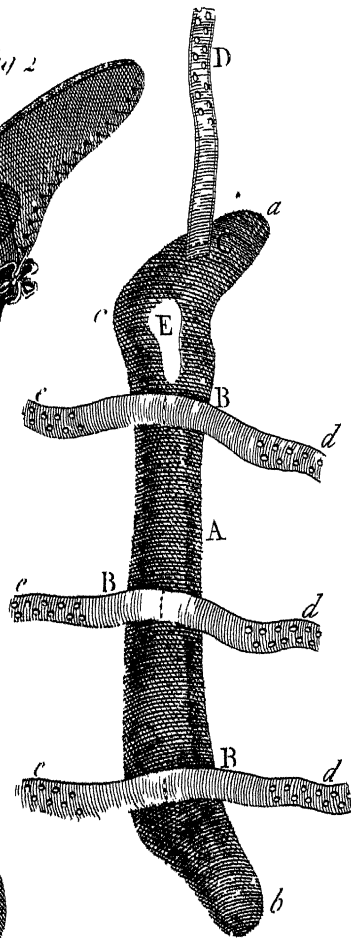
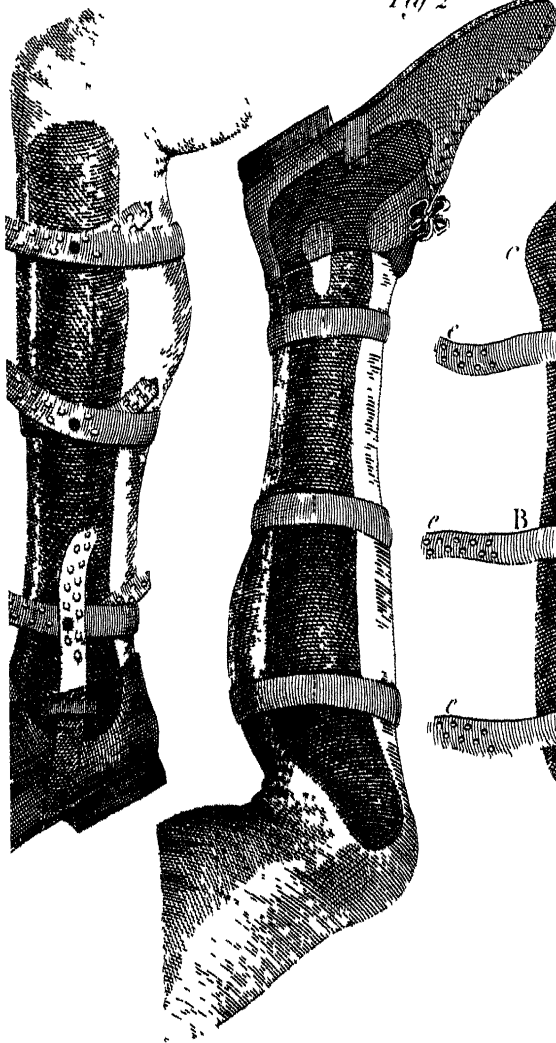
*Fig. 4.*



*Fig. 3.*



*Fig 2*



*Fig 1*



part of the splint, must be shorter than those on the posterior *c c c*, which are to surround the more muscular part of the leg.

C. A part to support the foot, from the point *a* to the heel *c*, five inches long, at an angle of sixty degrees.

D. The foot-strap, twelve inches long, sewed to the bottom of the under splint, within two inches of the point, to pass under the heel and through the leather loop on the upper splint, to the lowest pin.

E. An irregular oval hole, two inches long and almost one wide in the lowest part, but decreasing upwards, to receive the *malleolus externus*, or lower extremity of the fibula.

Fig. II. Represents the leg raised up, to shew the situation of the under splint, when properly applied.

Fig. III. Represents a fractured leg, when laid within the splints, according to the method I am about to recommend, having the stocking-foot (or sock) and shoe upon it: the darkest shade in this and Fig. II. being intended to shew that part of the splints within the shoe.

Fig. IV. A the upper splint. BBB the pins. C the leather loop to receive the foot-strap.

Fig. V. A many-tailed bandage, made of slips of Russia linnen, regularly increasing in length from twelve or fourteen to eighteen or twenty inches, according to the size of the leg. Each of these slips (being two inches broad) is so laid as to cover half the breadth of that which is underneath it (*viz.* one inch). Another slip, ten or twelve inches long, is sewed on the back so as to unite them all in the middle, making a bandage equally as firm as a circular one, and which

may be used without disturbing the leg. The narrowest part must be placed nearest to the heel. As the number of slips are to be lessened or increased according to the space necessary to be inclosed within them, I call this a many-tailed bandage, leaving the precise number to be determined according to the nature of the circumstances.

This has been used many years in St. Bartholomew's hospital, instead of the old eighteen-headed bandage, nevertheless, as it is not generally known, I hope this description will not be thought superfluous.

The three different sizes of splints above-mentioned will generally be sufficient; at least one or other of them may serve any leg of an adult till others can be provided.

The legs of children, as they are more round and less muscular, may be defended (nearly in the same manner) with the common wooden splints (properly bolstered), that are now made use of in St. Bartholomew's hospital; provided they are long enough to secure both articulations of the fractured bone.

When a surgeon is called to a fractured leg, at the place where the accident happened, let him lay the patient on the injured side, upon a flat surface; and raise the knee of the fractured limb towards the abdomen, bending at the same time the knee joint, so as to put the extensor muscles of the foot (which are the strongest) into a state of relaxation. He will then be enabled to replace the ends of the fractured bones, and restore them to their proper situation, without the customary strong extension of the limb; which is troublesome to the surgeon, painful to the patient, and  
apt

apt to bring on tension, spasms, and inflammation of the stretched muscles.

When the ends of the fractured bone are re-placed (which may often be done even without removing the shoe or stocking), let an under splint, of the most suitable size, be applied to the fibula, or outside of the leg; and, if it does not fit exactly, let it be made to do so by adding such compresses of tow, or thick flannel, as may be necessary for that purpose. Let also the upper splint be applied on the inside of the leg, so as almost to cover the tibia, on its whole length. The straps may then be fastened upon the pins sufficiently tight to secure the whole. This done, the patient may easily be removed in a sedan chair (having the cushion so raised that the leg may hang down without resting upon the bottom); or in a coach, with the limb supported by the hand of a surgeon, so that it may yield to every motion uniformly, or altogether as it were, whilst it swings in his hand: for it matters not how great the motion of the body be, provided the points of the fractured bone are secured from being moved the one against the other.

In this manner I have carried many patients from the place of the accident, over the London pavements, to their own homes at a great distance, without their having suffered any inconvenience from the motion, even where the fractures were compound.

When the patient is brought home and put to bed, (the bed having a mattress upon it), let the stocking be removed, and the proper remedies, with the many-tailed bandage, and the above splints, applied; observing the same directions as to posture, which I have already given: *viz.* that the patient be laid on his

his side with the broken leg undermost, the knee bent, and the thigh drawn up; instead of laying him on his back with the injured limb extended.

This posture is much more comfortable and convenient (rendering it less troublesome for the patient to ease himself, or be moved by others) and removes the foot and toes out of the way of being hurt by the weight of the bed-cloaths, so as to make a fracture-box or cradle unnecessary.

If the fracture be compound, the wound generally heals by the first intention; the great impediment (irritation) being prevented, of which I can produce many instances. Add to this, that the leg may be taken up with the whole apparatus, and the knee joint gently moved, as often as necessary, to prevent that stiffness, which always succeeds a case of this kind, and is attended with much pain and inconvenience, a long time after the leg is, otherwise, well. The patient may also be taken out of bed frequently, without pain or danger, if not very heavy or unweildy.

The under splint makes a safe and secure bed for the leg, whilst the upper part is dressing (if a compound fracture); and the leg may, by a steady hand, be supported also against the upper splint, and put into any posture that may be necessary to dress a sore on the fibula, or under part.

I do not always remove the shoe and stocking-foot; as well, because they serve to keep the part in perspiration, as, that the shoe adds steadiness to the limb, by the connection it may have to the inferior part of the instrument. Both the splints are, designedly, made narrow, to leave room for examination of the parts affected; lest any undue pressure should  
occasion

occasion pain : but, if it should be objected, that they are not sufficiently broad for a thick leg, that inconvenience may be remedied by putting a slip or pasteboard, or thin wood, between them on the fore-part of the leg, if thought necessary. The straps of the instrument are sufficient to secure the whole.

I made, with my own hand, the first of these instruments of strong pasteboard with iron plates riveted upon them; which succeeded very well. I have tried also various materials for the same purpose, such as strong hide leather, hardened with gliew; also wood, and plate-copper; any one of which will answer sufficiently, if well formed: nevertheless I shall prefer the pasteboard, *if made strong enough*, till I can meet with a workman, that will make them, accurately, of harder materials.

The same posture, that is recommended for broken legs, I have found equally serviceable with respect to broken thighs; and for the same reason: in which case the common wooden splint is as good as any other, provided it be long enough to secure both extremities of the fractured bone.

I have, through the whole of this description, made use of the terms upper and under splints, for the sake of being more easily understood: though perhaps the calling the one *tibiale*, and the other *fibularium*, would serve to distinguish them better, and give a more precise idea of the manner of their application; the first being placed so as to cover a great part of the tibia, and the second forming a safe bed for the fibula.



Received October 20, 1766.

X. *Account of a locked Jaw, and Paralysis, cured by Electricity: by Dr. Edward Spry, of Totness, in a Letter to Charles Morton, M. D. Sec. R. S.*

Read Feb. 19, 1767. **C**ATHARINE Smellidge, of Ditford, a girl aged eighteen, of a strong healthy constitution, took, at the accidental death of a friend, a great fright, and the next day (Easter-day, 1765) at his funeral, fell ill of very severe convulsive fits, which lasted, with slight intermissions, upwards of a month.

From the first attack, she never spoke, though otherwise sensible; soon after her jaws became quite fixt, so that she was obliged to be fed with thin panada, and the like, strained between her teeth, being not able to have them opened but a very little way, even by a wedge made for that purpose. She became likewise paralytic from her hip down, on the right side.

January 10, 1766. She consulted me, when I found her incapable of supporting herself without assistance, her leg and thigh of the right side very torpid with a loss of motion, and much more flaccid than the other, though not emaciated. She was incapable of uttering the least articulate sound, or even of having her teeth so far separated by the

*speculum oris*, as to admit my little finger between them.

The *masseter* and *temporal* muscles, from their contraction, felt vastly tense, and rigid, being particularly painful on our pressure thereon, or endeavour to open her mouth; the *genio-hyoidæi* muscles appeared alike circumstanced, and the *platysma-myoides* on the right side very often greatly convulsed.

Matters thus circumstanced, after every usual method judiciously administered by Mr. Guddrige of Brent, her surgeon, to little avail, I had but small hopes from medicine; therefore recommended electricity; on which account, she, having no opportunity of its being done in the country, came to her lodgings, taken in town for that purpose, on January 15, when, she being somewhat inclined to be plethoric, and her menses not hitherto interrupted, I ordered fourteen ounces of blood to be taken off, and the next day gave her a few slight (the feathered gnomon rising not above the horizontal) electrical shocks on the leg of the diseased side; she immediately felt an agreeable sensation therein.

This process was daily repeated, with a gradual increase of the *vis electrica*, sometimes *plus*, sometimes *minus*, electrifying her for six or seven days, by which time she became much stronger, and capable of walking alone tolerably well.

I now (she being, as to her jaw, and speech, as at first) several times full-charged her with the electric matter, discharging it alternately from the *masseters*, her temples, and under the chin; immediately on her parting with which, she, involuntarily, shook her

head, making her usual noise, in endeavouring to speak.

The next day, I fixed the conductor round her temples, and throat, and gave slight shocks, by touching sometimes her chin, othertimes her teeth or cheeks, with the communicant wire. This she disagreeably, though advantageously, felt, her jaws hereby admitting their being opened a little.

The next day, I (the gnomon being near erect) increased the shocks considerably, by which, though she very discontentedly bore them, she became capable of opening her mouth to the width of an inch, and of articulating an imperfect, though, with difficulty, an intelligible sound.

The next day, (the index quite perpendicular) she very reluctantly received several smart shocks, and at last unexpectedly (the air being very electric) to such a degree, as to deprive her of her senses; she becoming thereon, and remaining for half an hour, strongly convulsed.

The next day, after the first shock, she spoke so as to be tolerably well understood, telling us that the shocks were frequently vastly severe for her to bear; but that, as she was fully sensible of the advantage she had already received thereby, she would gladly submit to my will, in hopes of a further advantage.

She was even now incapable of bringing her tongue without her teeth, and of moving it without great difficulty, complaining it seemed very large, and heavy.

On inspecting her mouth, which she was able to open to almost its usual width, I discovered nothing particular,

particular, but an extraordinary turgescence, without induration, of the sublingual glands.

After this she received about twenty shocks daily on her tongue, and other parts, for a fortnight, by which time all her complaints were removed, and she returned home quite well, and has remained so ever since,

N. B. In the first week's experiments, the shocks were confined between her hip, and foot, of the right side; after that, on various parts, as judged requisite: her tongue, at its tip, became very red, and tender, after the first electrization, its *papillæ* appearing very prominent; and its subjacent glands soon lessened their bulk, her mouth running greatly with saliva: her pulse, with a shock or two, generally quickened twelve or fourteen times per minute. She, after grown tolerably well, immediately on having a smart electrical stroke, frequently became, for some small time, as paralytic as ever on her right side; and sometimes, thereon, had a return of her fits, the going off of which were attended with profuse sweats. Her blood appeared of a good texture, otherwise than giving off a little more than its due proportion of latex.

Received December 11, 1766.

XI. *Experiments on Rathbone-Place Water :*  
*By the Hon. Henry Cavendish, F. R. S.*

Read Feb. 19, 1767. **D**R. Lucas has given a short examination of this water in the first part of his treatise of waters. It is the produce of a large spring at the end of Rathbone-place, and used a few years ago to be raised by an engine for supplying part of the town. The engine is now destroyed ; but there is a pump, nearly in the same situation, which yields the same kind of water. It is the water of this pump, which was used in these experiments.

Most waters, though ever so transparent, contain some calcareous earth, which is separated from them by boiling, and which seems to be dissolved in them without being neutralized by any acid, and may therefore not improperly be called their unneutralized earth. The following experiments were made chiefly with a view of enquiring into the cause of the suspension of this earth, for which purpose this water seemed well adapted ; as it contains more unneutralized earth than most others.

These experiments were made towards the latter end of September 1765, after a very dry summer ; whereby the water was most likely more impregnated with saline and other matters than it usually is.

The

The water, at the time I used it, looked rather foul to the eye. On exposing some of it for a few days to the open air, a scurf was formed on its surface, which was nothing else but some of the unneutralized earth separated from the water. On dropping into it a solution of corrosive sublimate, it grew cloudy in a few seconds; it quickly became opaque, and let fall a sediment. This is a property, which I believe does not take place, in any considerable degree, in most of the London waters.

#### EXPERIMENT I.

494 ounces of this water were distilled in a copper still, till about 150 oz. were drawn off. A good deal of earth was precipitated during the distillation, which being collected and dried, weighed 271 grains. It proved to be entirely a calcareous earth, except a small part, which was magnesia. This I found in the following manner. A little of this earth, being mixed with spirit of salt, dissolved entirely; which shews it to consist solely of an absorbent earth, but does not shew whether it is a calcareous earth or magnesia. The remainder was saturated with oil of vitriol: a great deal of matter remained undissolved, which, as the earth was shewn to be entirely of the absorbent kind, must have been selenite, or a calcareous earth saturated with the oil of vitriol. The clear liquor strained from off the selenite yielded on evaporation only eighteen grains of solid matter, which proved to be Epsom salt; so that all the earth, except that contained in the eighteen grains of Epsom salt, must have been of the calcareous kind. That contained in the Epsom salt is well known to be magnesia.

The

The water remaining after distillation, and from which the earth was separated, was evaporated, first in a silver pan, and afterwards in a glass cup, till it was reduced to about three ounces. Not the least earth was precipitated during the evaporation, till it was reduced to a small quantity; there then fell 39 grains, which were entirely selenite: so that all the unneutralized earth in the water was separated during the distillation. The liquor thus evaporated was of a reddish colour, like an infusion of foot.

Many waters contain a good deal of neutral salt composed of the nitrous acid united to a calcareous earth; the most convenient way of ascertaining the quantity of which, is to drop a solution of fixed alkali into the evaporated water, till all the earth is precipitated; whereby this salt is changed into true nitre, and is capable of being crystallized. For this reason, some fixed alkali was dropped into the evaporated water till it made no farther precipitation. The earth precipitated thereby weighed thirty-six grains, and was entirely magnesia. The liquor was then farther evaporated, but no nitre could be made to shoot: being then evaporated to dryness, it weighed 256 grains. It gave not the least signs of containing any nitrous salt, either by putting some of it upon lighted charcoal, or by making a match with a solution of it, but appeared to be a mixture of sea salt and vitriolated tartar, or some other salt composed of the vitriolic acid. As I have heard of no other London water, that has been examined with this view, but what has been found to contain a considerable proportion of nitrous salt, it seems very remarkable that this should be entirely destitute of it. I now proceed

proceed to the experiments made on the distilled water.

The distilled water, especially that part of it which came over first, became opaque, and let fall a precipitate, on dropping into it a solution of sugar of lead. It also became opaque by the addition of corrosive sublimate, much in the same manner that the plain water did before distillation.

It was found, by dropping into it a little acid of vitriol and committing it to evaporation, to contain a small quantity of volatile alkali; as it left four grains of a brownish salt, which being re-dissolved in water, yielded a smell of volatile alkali on the addition of lime. It is doubtless this volatile alkali, which is the cause of the precipitate, which the distilled water makes with sugar of lead and corrosive sublimate.

What first suggested to me that the distilled water contained a volatile alkali, was the distilling some of it over again in a retort; whereby the first runnings were so much impregnated with volatile alkali, as to turn paper dyed with the juice of blue flowers, to a green colour, and in some measure to yield a smell of volatile alkali.

In the foregoing experiment, the salt procured from the distilled water was perfectly neutral; so that the quantity of acid employed was certainly not more than sufficient to saturate the alkali, but it may very likely have been less; as in that case the superfluous volatile alkali would have flown off in the evaporation. The following experiment shews pretty nearly the quantity of volatile alkali in the distilled water.



## EXPERIMENT II.

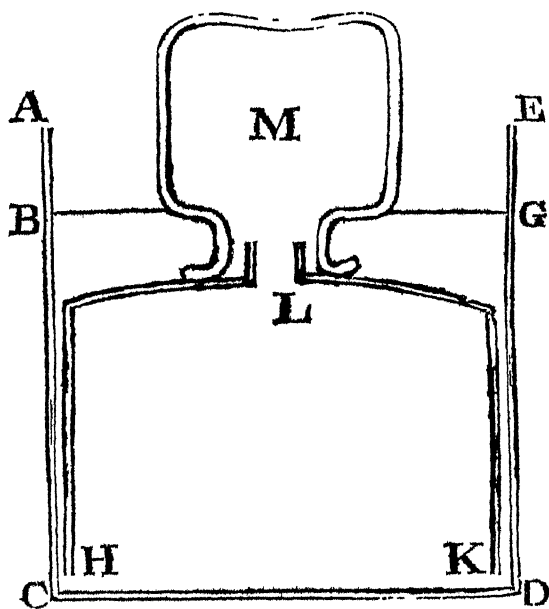
1128 ounces of Rathbone place water were distilled in the same manner as the former. The distilled water was divided into two parcels, that parcel which came over first weighing 121 ounces, the other 146. A preparatory experiment was first made, in order to form a judgement of the comparative strength of each parcel, and also of the quantity of acid which it would require to saturate them. This was done by dropping sugar of lead into each parcel till it ceased to make a precipitate. It was judged from hence that the first parcel contained about  $2\frac{1}{2}$  times as much volatile alkali as an equal quantity of the second. Into 30 ounces of the first parcel, mixed with as much of the second, was then put 43 grains of oil of vitriol, which was supposed to be about  $\frac{1}{2}$  more than sufficient to saturate the alkali therein. The mixture was then evaporated. When reduced to a small quantity, it was found to be rather acid: sixteen grains of volatile sal ammoniac were therefore added, which seemed nearly sufficient to neutralize it. Being then evaporated to dryness, it left sixty-six grains of a brownish salt, which dissolved readily in water, leaving only a trifling quantity of brown sediment. A little of this salt was found to make no precipitate on the addition of fixed alkali, and the remainder, being boiled with lime, was converted into selenite; a sure sign that the salt was merely vitriolic ammoniacal salt. The volatile alkaline salt contained in sixty six grains of vitriolic ammoniacal salt is  $58\frac{1}{2}$  grains; from whence deducting sixteen grains, the weight of the volatile sal ammoniac added, it appears that the distilled

water

water used in this experiment contains  $42\frac{1}{2}$  grains of volatile salt; and therefore the whole quantity of volatile salt driven over by distillation seems to be about sixty-eight grains, which, as the second parcel was so much weaker than the first, is probably nearly the whole volatile alkali contained in the water.

### EXPERIMENT III.

Dr. Brownrig, in a paper printed in the Philosophical Transactions, for the year 1765, shews that a great deal of fixed air is contained in Spa water. This induced me to try whether I could not find any in that of Rathbone-place; which I did by means of the contrivance represented in the drawing.



ACDE represents a tin pan, filled with Rathbone-place water as high as BG. HKL is another tin pan, within the first, in the manner of an inverted funnel, and made in such a manner as to leave as little room as possible between that and the sides of the outward vessel. M represents a bottle, full of the same water, inverted over the mouth of the funnel. By this means, as fast as the air is disengaged by heat from the water within the funnel, it must necessarily rise up into the bottle. The Rathbone-place water, put into the vessel, weighed 411 ounces, the funnel held 353 ounces. A bottle full of water being inverted over the mouth of the funnel, as in the figure, the water was heated, and kept boiling about  $\frac{1}{4}$  of an hour. As soon as one bottle was filled with air, it was removed by putting a small ladle under its mouth, while under water, and set with its mouth immersed in the same manner in another vessel of water, taking care not to suffer any communication between the included air and the outward air during the removal. At the same time, another bottle full of water was inverted over the mouth of the funnel, in the same manner as the former. It was not easy telling how much air was discharged from the water; as the air in the bottles, when first removed, was hot and expanded; and, before I could be sure it was cold, there was some of it absorbed by the water: but there seemed to be above 75 ounce measures discharged, scarce twenty of which arose before the water began to boil. The water continued discharging air after the experiment was discontinued. In about a day's time, much the greatest part of the air was absorbed, scarce sixteen ounce measures remaining. That which was absorbed appeared to be fixed air, as the water which had absorbed

absorbed it made a precipitate with lime-water. But, in order to absorb all the fixed air more perfectly, the air which remained not absorbed was transferred into another bottle of water, in the manner described in my first paper on factitious air, page 142 of the preceding volume. This bottle was then set with its mouth immersed in a bottle of sope-leys; after which, by shaking the bottle, the sope-leys was mixed with the included water; whereby the air in the bottle was brought in contact with the sope-leys, which is well known to absorb fixed air very readily. By this means the air was reduced to  $8\frac{3}{4}$  ounce measures. A small vial being filled with equal quantities of this and inflammable air, and a piece of lighted paper applied to its mouth, it went off with as loud a bounce, as when the same vial was filled with equal quantities of common air and inflammable air. The specific gravity of the remainder was tried by a bladder, in the manner described in the above-mentioned paper; as well as could be judged from so small a quantity, it was just the same as that of common air. From these two circumstances, I think we may fairly conclude that this unabsorbed part was intirely common air; consequently the air discharged from the Rathbone-place water consisted of  $8\frac{3}{4}$  ounces of common air and about 66 of fixed air. The air which was discharged before the water began to boil contained much more common air, than that which was discharged afterwards; that which was discharged towards the latter end seeming to contain scarce any but fixed air.

As so much fixed air is discharged from this water by boiling, it seemed reasonable to suppose, that the distilled water should contain fixed air. Accordingly I found it to make a precipitate with lime-water.

## EXPERIMENT IV.

The following experiment shews that the fixed air was not generated during the boiling, but was contained in the water before. Into 30 ounces of Rathbone-place water was poured some lime-water, which immediately made a precipitate. More lime-water was added, till it ceased to make any farther precipitate. It required  $20\frac{1}{4}$  ounces. The precipitated earth being dried weighed 39 grains.

The unneutralized earth contained in 30 ounces of Rathbone-place water is  $16\frac{1}{2}$  grains, and the earth contained in  $20\frac{1}{4}$  ounces of lime-water (as was found by precipitating the earth by volatile sal ammoniac) is 21 grains. Therefore the earth precipitated from the mixture of Rathbone-place water, and lime-water, is about equal to the sum of the weights, of the earth contained in the lime-water, and of the unneutralized earth in the Rathbone-place water; and consequently all the unneutralized earth seems to be precipitated from Rathbone-place water by the addition of a proper quantity of lime-water. But a more convincing proof that this is the case, is that the clear liquor, after the precipitate had subsided, did not deposit any earth on boiling, or become in the least cloudy on the addition of fixed alkali; whereas Rathbone-place water in its natural state becomes opake thereby. It might perhaps be expected, that the clear liquor should still make a precipitate on the addition of fixed alkali, though the unneutralized earth is precipitated; as in all probability there is still a good deal of earth remaining in it in a neutralized state. The reason why it does not, seems to be, that the remaining earth is most likely intirely magnesia; and Epsom salt,

salt, when dissolved in a great quantity of water, does not make any precipitate on the addition of fixed alkali.

There is great reason to suppose that the earth precipitated on mixing the Rathbone-place water and lime-water, was very nearly saturated with fixed air, i. e. that it contained very near as much fixed air, as is naturally contained in the same quantity of calcarious earth. If so, 30 ounces of Rathbone-place water contain as much fixed air as 39 grains of calcarious earth; whereas the unneutralized earth, in that quantity of water, is only  $16 \frac{1}{2}$  grains; so that Rathbone place water contains near  $2 \frac{1}{2}$  times as much fixed air as is sufficient to saturate the unneutralized earth in it.

It seems likely from hence, that the suspension of the earth in the Rathbone-place water, is owing merely to its being united to more than its natural proportion of fixed air; as we have shewn that this earth is actually united to more than double its natural proportion of fixed air, and also that it is immediately precipitated, either by driving off the superfluous fixed air by heat, or absorbing it by the addition of a proper quantity of lime water.

Calcareous earths, in their natural state, i. e. saturated with fixed air, are totally insoluble in water; but the same earths, entirely deprived of their fixed air, i. e. converted into lime, are in some measure soluble in it; for lime-water is nothing more than a solution of a small quantity of lime in water. It is very remarkable, therefore, that calcareous earths should also be rendered soluble in water, by furnishing them with more than their natural proportion of fixed air, i. e. that they should be rendered soluble, both by depriving them of their fixed air,  
and

and by furnishing them with more than their natural quantity of it. Yet, strange as this may appear, the following experiments, I think, shew plainly that it is the real case.

#### EXPERIMENT V.

In order to see whether I could suspend a calcareous earth in water, by furnishing it with more than its natural proportion of fixed air, I took 30 ounces of rain water, and divided it into two parts: into one part I put as much spirit of salt, as would dissolve  $30 \frac{3}{4}$  grains of calcareous earth, and as much of a saturated solution of chalk, in spirit of salt, as contained 20 grains of calcareous earth: into the other part I put as much fixed alkali, as was equivalent to  $46 \frac{8}{9}$  grains of calcareous earth, i. e. which would saturate as much acid. This alkali was known to contain as much fixed air as 39 grains of calcareous earth. The whole was then mixed together and the bottle immediately stopped. The alkali was before said to be equivalent to  $46 \frac{8}{9}$  grains of calcareous earth, and was, therefore, sufficient to saturate all the spirit of salt, and also to decompose as much of the solution of chalk as contains  $16 \frac{1}{2}$  grains of earth. This mixture, therefore, supposing I made no mistake in my calculation, contained  $16 \frac{1}{2}$  grains of unneutralized earth, with as much fixed air as is contained in 39 grains of calcareous earth; which is the quantity which was found to be in the same quantity of Rathbone place water. The mixture became turbid on first mixing, but the earth was quickly re-dissolved on shaking, so that the liquor became almost transparent. After standing some time, a slight sediment fell to the bottom, leaving the liquor perfectly transparent.

transparent. The mixture was kept three or four days stopped up, during which time it remained perfectly clear, without depositing any more sediment. The clear liquor was then poured off from the sediment, and boiled for a few minutes, in a Florence flask; it grew turbid before it began to boil, and discharged a good deal of air; some earth was precipitated during boiling, which being dried weighed 13 grains.

This shews that there was really, at least 13 grains of earth suspended in this mixture, without being neutralized by any acid; the suspension of which could be owing only to its being united to more than its natural proportion of fixed air. But, as a further proof of this, I made the following experiment.

#### EXPERIMENT VI.

I took the same quantities of rain water, solution of chalk, spirit of salt, and fixed alkali, as in the last experiment, but mixed them in a different order. The fixed alkali was first dropped into the spirit of salt, and when the effervescence was over, was diluted with  $\frac{1}{2}$  the rain water. The solution of chalk was then diluted with the remainder of the rain water, the whole mixed together, and the bottle immediately stopped, and shook vehemently. A precipitate was immediately formed on mixing, which could not be re-dissolved on shaking.

It must be observed, that, in the first of the two foregoing experiments, all the fixed air contained in the alkali was retained in the mixture, none being lost by effervescence; whereas, in the last experiment, the greatest part of the fixed air was dissipated in the effervescence; no more being retained than  
what



what was contained in that portion of the fixed alkali, which was not neutralized by the acid; and consequently the unneutralized earth, in the mixture, contained not much more fixed air than what was sufficient to saturate it. As the latter of these mixtures differed no otherwise from the former, than that it contained less fixed air; the suspension of the earth in the former must necessarily be owing to the fixed air.

In the two foregoing experiments the water contained, besides the unneutralized earth, and fixed air, some sal sylvii, and a little solution of chalk in the marine acid; which, it may be supposed, contributed to the suspension of the earth: but the following experiment shews that a calcarious earth may be suspended in water, without the addition of any other substance than fixed air.

#### EXPERIMENT VII.

A bottle full of rain water was inverted into a vessel of rain water, and some fixed air forced up into the bottle, at different times, till the water had absorbed as much fixed air as it would readily do; 11 ounces of this water were mixed with  $6\frac{1}{2}$  of lime water. The mixture became turbid on first mixing, but quickly recovered its transparency, on shaking, and has remained so for upwards for a year.

This mixture contains 7 grains of calcareous earth; and, from a subsequent experiment, I guess it to contain as much fixed air, as there is in 14 grains of calcareous earth.

#### EXPERIMENT VIII.

Least it should be supposed, that the reason why the earth was not precipitated in the foregoing experiment,

ment, was, that it was not furnished with a sufficient quantity of fixed air, the following mixture was made, which contains the same proportion of earth as the former, but a less proportion of fixed air:  $4\frac{1}{2}$  ounces of the above-mentioned water, containing fixed air, were diluted with  $6\frac{1}{4}$  of rain water, and then mixed with  $6\frac{1}{2}$  ounces of limewater. A precipitate was immediately made on mixing, which could not be re-dissolved on shaking.

#### EXPERIMENT IX.

I made some experiments to find whether the unneutralized earth could be precipitated from other London waters, by the addition of lime water, as well as from Rathbone-place water. It is necessary for this purpose, that the quantity of lime water should be adjusted very exactly; for, if it is too little, it does not precipitate all the unneutralized earth; if it is too great, some of the earth in the lime water remains suspended. For this reason, as I found it almost impossible to adjust the quantity with sufficient exactness, I added such a quantity of lime water, as I was well assured, was more than sufficient to precipitate the whole of the unneutralized earth; and when the precipitate was subsided, decanted off the clear liquor, and exposed it to the open air, till all the lime remaining in the water was precipitated, by attracting fixed air from the atmosphere. The clear liquor was then decanted and evaporated, which is much the most exact way I know of seeing whether any unneutralized earth remains suspended in the water. The result of the experiments was as follows:

200 ounces of water, from a pump in Marlborough-street, were mixed with 38 ounces of lime water. The earth precipitated thereby weighed 38 grains. The clear liquor, exposed to the air, and evaporated in a silver pan till it was reduced to 6 or 7 ounces, deposited no more than 2 or 3 grains of unneutralized earth.

A like quantity of the same pump water, evaporated by itself without the addition of lime water, deposited about 19 grains of unneutralized earth.

200 ounces of water, from a pump in Hanover-square, being mixed with 67 ounces of lime water, the precipitate weighed 93 grains. The clear liquor, treated in the same way as the former, deposited about 2 grains of earth. 200 ounces of the same water, evaporated by itself, deposited 28 grains of earth.

The same quantity of water from a pump in St. Martin's church-yard, being mixed with 82 ounces of lime water, the precipitate weighed 108 grains. The clear liquor deposited scarce any unneutralized earth on evaporation.

The same quantity of water, evaporated by itself, yielded 45 grains of unneutralized earth.

The way, by which I found the quantity of unneutralized earth deposited on evaporation, was, after having decanted the clear liquor, and washed the residuum with rain water, to pour a little spirit of salt into the silver pan, which dissolves all the calcareous earth, but does not corrode the silver. Then, having separated the solution from the insoluble matter, the earth was precipitated by fixed alkali.

In this way of finding the quantity of unneutralized earth, care must be taken to add very little more acid

acid than is necessary to dissolve the unneutralized earth, and to use as little water in washing out the solution as possible; for otherwise a good deal of the selenite, which is deposited in the evaporation of most water, will be dissolved; the earth of which will be precipitated by the fixed alkali, and by that means make the quantity of unneutralized earth appear greater than it really is.

It appears from these experiments, that the unneutralized earth is intirely precipitated from these three waters, by the addition of a proper quantity of lime water; as the trifling quantity found to be deposited, on the evaporation of two of them, most likely proceeded only from not exposing the water to the air, long enough for all the lime to be precipitated. So that I think it seems reasonable to conclude, that the unneutralized earth, in all waters, is suspended merely by being united to more than its natural proportion of fixed air.

To return to Rathbone-place water; it appears from the foregoing experiments, that one pint of it, or 7315 grains, contains, first, as much volatile alkali as is equivalent to about  $\frac{2}{3}$  grains of volatile sal ammoniac: secondly, 8  $\frac{1}{2}$  grains of unneutralized earth, a very small part of which is magnesia, the rest a calcareous earth: thirdly, as much fixed air, including that in the unneutralized earth, as is contained in 19  $\frac{3}{4}$  grains of calcareous earth: fourthly, 1  $\frac{1}{2}$  of selenite: fifthly, 7  $\frac{1}{4}$  of a mixture of sea salt, and Epsom salt; and the whole solid contents of 1 pint of the water is 17  $\frac{1}{2}$  grains.

One pint of water, from the pump in Marlborough-street, contains 1  $\frac{1}{2}$  grains of unneutralized

earth, and as much fixed air as is contained in  $2 \frac{1}{10}$  grains of calcareous earth.

The same quantity of water, from the pump in Hanover-square, contains  $2 \frac{1}{10}$  grains of unneutralized earth, with as much fixed air as is contained in  $7 \frac{1}{10}$  of earth.

The same quantity of water, from St. Martin's Church-yard, contains  $3 \frac{1}{10}$  grains of unneutralized earth, with as much fixed air as is contained in  $8 \frac{1}{10}$  of earth.

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Received November 18, 1766.

XII. *Description of a Meteor seen at Oxford, October 12, 1766. In a Letter to Charles Morton, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Dear Sir,

Read Feb. 26, 1767. **T**HE Reverend Dr. Sharp coming into Christ-Church common-room out of the great quadrangle, on Saturday, October 12, 1765, about  $8^h 30'$  P. M. informed the company there, that he had seen some remarkable *Aurora Boreales* a few minutes before. But, as such phenomena









mena are common enough here, they gave little attention to the information. However, being upon the terrace, about 8<sup>h</sup> 45' P. M. I discovered (See Tab. V. \*) a broad luminous arch, in the northern part of the hemisphere, extending from E. to W. almost terminated by the horizon, and somewhat less than a semicircle. The upper or exterior limb of this arch, together with a certain portion of the lucid adjoining tract, was white and resplendent; but the brightness gradually decreased as it approached the lower or interior limb, which was so fuscous and obscure, that it seemed scarce distinguishable from the clouds that were contiguous to it. For about five minutes, the lustre remained pretty strong and vivid, and the meteor without any visible change or variation; but, after the expiration of that short term, the arch began to grow faint, and in one or two minutes more, as near as I can guess, totally disappeared.

How long this meteor had been formed, when I first observed it, I cannot take upon me to say; but I believe it was then, and perhaps for some time had been, upon the decline. It was seen by the Reverend Mr. Selstone and the Reverend Mr. Best, chaplains of Christ-Church, as well as by other members of the University, not without some degree of wonder and surprize. The crepusculum, or illustration of the atmosphere, which sometimes precedes such meteors as that described here, and even continues long after their extinction, might perhaps have remained till ten or eleven o'clock; which if we admit, this crepusculum may not improbably be considered as the same phenomenon with "the surprizing  
" bright \* luminous appearance visible at London in

\* LLOYD'S *Evening Post*, &c. No. 1289. p. 365.

“ the hemisphere from the East to the West, about “ ten o’clock, which lasted about an hour,” the same night, or at least as something similar to it. In either of which cases, the atmosphere at London will be allowed to have been in a proper disposition for the production of this species of meteors, and impregnated with the same kind of luminous vapour that occasioned the phenomenon mentioned here.

The singularity of this meteor was fixed by the gradual and regular diminution of its resplendency between the upper and lower limbs, an instance of which I never observed before. This continued from the time I first discovered the arch almost to the very moment of its extinction. The limbs of the zone forming this arch were, however, very well defined; insomuch that the regularity of its figure, by the gradual decrease of brightness, was not in the least impaired. The evening was somewhat cloudy, but still and calm, and several of the stars appeared. As I have not met with a description of such a phenomenon, in any of the physiological papers consulted by me on this occasion; I have taken the liberty to transmit you the foregoing account of the meteor seen here, on Saturday, October 12, 1766, to be communicated to the Royal Society, hoping it may prove not altogether unacceptable to the members of that most learned and illustrious body. I am, with great regard,

Dear Sir,

Your most obliged  
humble Servant,

Christ-Church, Oxon.  
Nov. 7, 1766.

John Swinton.  
Received

Received November 18, 1766.

XIII. *Some Observations on Swarms of Gnats, particularly one seen at Oxford, August 20, 1766. In a Letter to Charles Morton, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

Dear Sir,

Read Feb. 26, 1767. **T**HE gnats have been more numerous, as well as more noxious, here, during the months of July, August, and September, 1766, than perhaps they were ever known to be before in the memory of man. So many myriads of them have sometimes occupied the same part of the atmosphere, in contiguous bodies, that they have resembled a very black cloud, greatly darkened the air, and almost totally intercepted the solar rays. The repeated bites likewise of these malignant insects have been so severe, that the legs, arms, heads, and other parts, affected by them, in many persons, have been swelled to an enormous size. The colour also of these parts, at the same time, was red and fiery, perfectly similar to that of some of the most alarming inflammations.

As the natural history of these troublesome insects is sufficiently known, having been given us, with great accuracy

accuracy and precision, by Monfct, Swammerdam, Reaumur, and others, it would be superfluous to expatiate upon them here. But I cannot forbear taking notice of one very remarkable property of these little mischievous animals, which lately presented itself to my view, and which has not yet perhaps been duly attended to by any naturalist. Being in the garden belonging to the Fellows of Wadham College, with the Reverend Mr. Allen, Fellow of that house, on Wednesday, August 20, 1766, about half an hour before sun-set, such an immense number of gnats filled the atmosphere, in which we breathed, as I had never seen before. We both of us also then observed six columns, formed intirely of these insects, ascending from the tops of six boughs of an apple-tree, in another garden, separated from that we were in by a partition-wall, to the height of at least fifty or sixty feet. Two of these columns seemed perfectly erect and perpendicular, three of them oblique, and one approached somewhat towards a pyramidal form. That bodies of gnats, in figure a little similar to the pillars mentioned here, are now and then to be seen, we learn from an ingenious \* author; but that these bodies ever ascend fifty or sixty feet, has not, I believe, been yet observed by any zoologer or natural historian.

It may not be improper to remark, that some of these gnats had their bodies greatly distended, and swoln much beyond their usual size, by the uncommon quantities of blood they had imbibed. One of them, in particular, being killed at the castle here, seemed considerably larger than any of the rest, and

\* Tho. Moufet, *Insector. sive Minimor. Animal. Theatr.* c. xiii. p. 82. Londini, 1634.

had as much blood expressed from it as stained or besmeared part of a wall three or four inches square.

I have been informed by the Reverend Dr. Wyndham, Warden of Wadham College in this University, that, about thirty years ago, many columns of gnats were perceived to rise from the top of the steeple of the cathedral church at Salisbury, by a considerable number of people. He likewise declared, that these columns were seen both by himself and the Reverend Dr. John Clarke, then Dean of Salisbury; that, at a small distance, they resembled smoke; and that this at first occasioned a sort of alarm, many believing that the church was on fire.

I shall only beg leave at present to add, that the Reverend Mr. Berkeley, Student of Christ-Church, as he assured me, was entertained with an appearance similar to that I had seen, the same evening, in the country, at a small distance from Oxford; that some of the bodies of gnats perceived by him, unless he was deceived, ascended much higher than those I had observed; and that I am, with all due sentiments of respect, Sir,

Your much obliged,  
and most obedient,  
humble servant,

Christ-Church, Oxon.  
Nov. 15, 1766.

John Swinton.

XIV. *A Description of the Andrachne, with its Botanical Characters: By G. D. Ehret, F. R. S.*

Read Feb. 26, 1767. FROM a short and crooked stem go off irregularly several branches bending in various directions; but the younger shoots mostly pointing upwards. The height of the shrub is now about four feet.

The stem and branches are of different colours at different seasons. In the spring, they appear of a greenish cinnamon colour; this is gradually heightened to almost a red during winter; towards the end of which, the epidermis peels off, and the new bark exhibits the like appearance as it had the spring before.

On the extremities of these branches, the shoots of the preceding year, which are of a deep red colour, are many leaves of different sizes, placed irregularly; the largest leaves were in length, when the figure was drawn, about four inches, and two inches and an half in breadth, of an oval figure: they are mostly entire, though the edges of some are lightly serrated: their surface is smooth and lively, but not glossy or shining. They are supported on the branches by footstalks about an inch long, of a red colour and smooth.

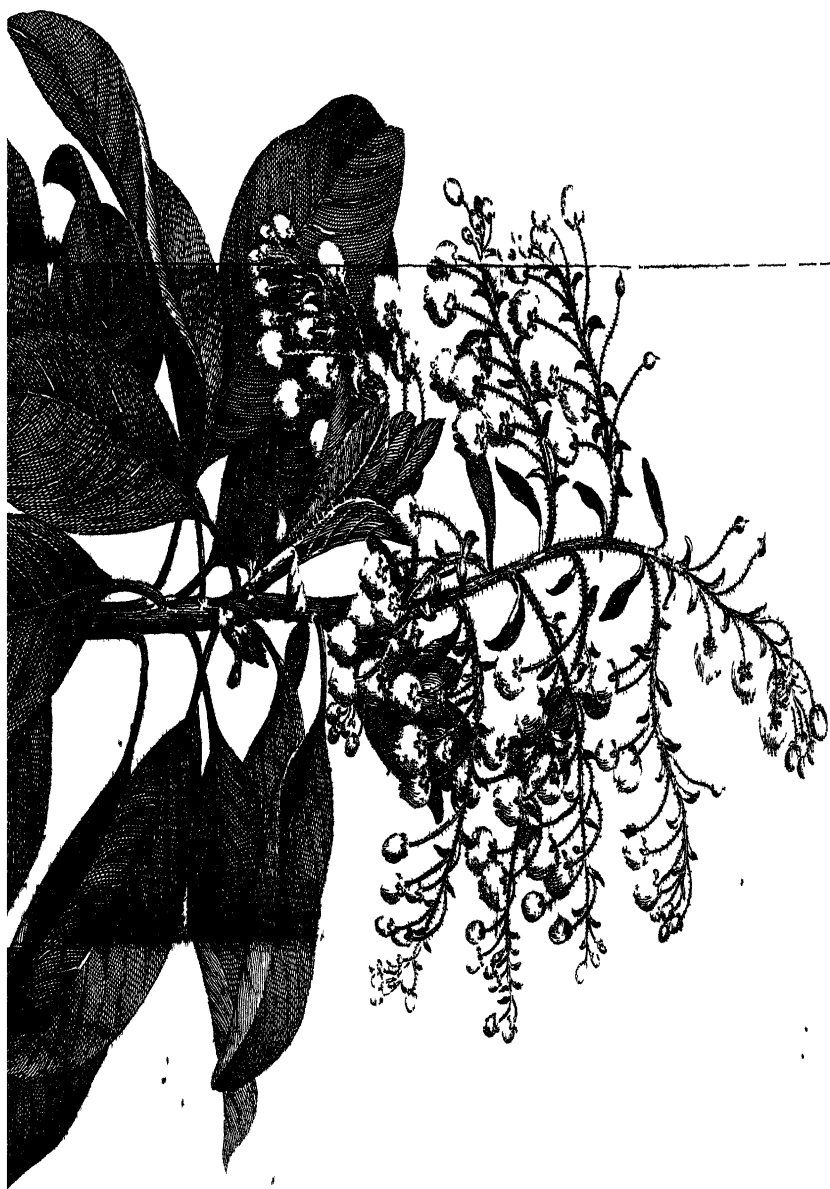
The young leaves, at their first appearance, are of a faintish green with a cast of yellow yet beautifully shaded with red: their footstalks and middle rib are



ARBUTUS *(Andrachne) fruticosa, foliis ovatis, subtus  
ovatis integris, et serratis, Bacca bacculosa pendulorum.*









then hoary, but they lose this appearance as they grow older.

This very rare shrub produced its flowers, for the first time in England, in the garden of Dr. John Fothergill, at Upton near Stafford in Essex, May 1766. The principal spikes of flowers in this species of *arbutus* are erect, producing many side ones in a horizontal direction, their extremities inclining downwards. Each of these simple ramifications contain many white globular flowers, hanging on long hoary glutinous pedunculi, ~~which are situated alternately.~~ These spikes of flowers, forming a kind of loose tuft with the bright bunches of leaves, form an elegant appearance.

#### Characters of the Flower.

#### T A B. VI.

Fig. *a.* represents a side view of the flowers; they are of a globular shape, and open into five obtuse reflex laciniae, in the manner of the common *arbutus*.

Fig. *b.* a back view of the flower, upon which appears the calyx spread open, and closely adhering to the flower; it consists of five oval pointed leaves or divisions; around this calyx appear on the corolla ten visible nectaria.

When these flowers drop off, the calyx closes up, and embraces the tender germ. See Fig. *c.*

Fig. *d.* represents a flower separated from the calyx; it is inserted at the base of the germen. The ten nectaria, which are somewhat swollen or raised from the corolla, and have transparent appearances, are

also discoverable, whilst the magnified figure *e* lays the parts more distinctly in view. This is a remarkable character in this flower.

Fig. *f*. exhibits the flower laid open: it is smooth without, and hoary within; it contains ten stamina, which are inserted at the base of the flower, their filaments and apices embrace half the style.

Fig. *g*. two stamina magnified, the base whereof is a tender fleshy substance, hoary and of a club-like shape; this diminishes gradually into a filament, upon which is situated a singular anthera; this anthera bursts at two apertures (as the figure represents), and disperses its farinaceous dust towards the style: from the top of this apex, comes forth, at the opposite side, two crooked forked horns, bending downwards in length of the anthera.

Fig. *b*. the germen or rudiment magnified. This is hoary, its base consists of a red fleshy substance, with ten obtuse angles. The style supports a small globular stigma, and does not exceed the length of the flower.

Fig. *i*. represents a horizontal view of the germen, as observed through a lens; it has five regular locuments or cells, though seemingly but one seed; but by a closer inspection, there appeared several embryo seeds in each cell.

Fig. *k*. a dried fruit or berry of the andrachne in its natural size, with an horizontal section. This fruit, which is tuberculous, I drew from a specimen consisting of the whole branch, leaves, flower-spikes, with many ripe berries which was brought from Aleppo, by Dr. Alexander Russell; all which I examined and described at that time for my own satisfaction, and find

find them to agree exactly with the recent shrub above described. It likewise seems worthy of observation, that the plants raised by the gardeners by grafting or inarching the andrachne upon the common arbutus, which is the method chiefly used in propagating this elegant shrub, differ considerably from the plants raised from seed, particularly in this, that the young branches, and the footstalks of the leaves, are very hairy, and the leaves themselves are all without exception deeply serrated like the arbutus. Dr. Russell also informs me, that the outer bark of the old stem and branches abroad, are for some months of the year of as beautiful a crimson, as the young shoots are here described to be, and doubts not but it will be so in this country, as the shrub grows older \*.

\* It may not be improper to mention, that the flower spike above described, with the glandular prominences, which were the rudiments of future flowers, made their appearance soon after Midsummer 1765: they advanced very slowly during the remains of summer; stood the winter under a slight cover, and made no great progress, till within a month of their flowering.

That plant, which produced these flowers, was one of several, which J. Gordon, of Mile-end, was fortunate enough to raise from seed, sent by Dr. Russell from Aleppo in 1754; and that this should be the only plant which has hitherto produced flowers, is probably owing principally to its having been divers times transplanted.

J. F.

Received December 20, 1766.

*XV. History of a Fœtus born with a very imperfect Brain; to which is subjoined a Supplement of the Essay on the Use of Ganglions, published in Philos. Trans. for 1764: By James Johnston, M. D.*

Read March 5, 1767. **I**N October 27, 1765, a monstrous birth

was brought me by a midwife of this place. It was a female child come to its full time, in which the whole skull excepting its basis was wanting: this was covered with something which had the appearance of red flesh. I found it to consist of different membranes; and in a small depression, in a back part of the basis of the skull, lay the brain, such as it was, not exceeding the size of the kernel of a filberd nut, flaccid and membranous. I could not have positively pronounced it brain, had I not traced its continuation into spinal marrow, down the channel of the vertebræ. The eyes were perfect and sound. The optic nerve of one eye I examined, though not large enough, yet in thickness was almost equal to one third of the spinal marrow, which was too small likewise.

Upon opening the breast and abdomen, all the organs contained therein seemed in structure perfect, properly situated and full grown. The heart in particular was plump and strong. This infant had not  
breathed,

breathed; its lungs, which were perfect, sunk in water: yet the mother and midwife felt it active and strong just before delivery.

This child had tongue, nostrils, eyes, and ears, and every other part, excepting the brain, perfect and plump, as in the healthiest infants come to their full time.

Many births similar to this, in most circumstances, are recorded in the Transactions of the Royal Society, N°. 99. 226. 228. 242.

1. Such of them as were born alive, died soon after birth, though lively and strong in the womb, and perfect in all parts, the brain and skull excepted.

2. In that of which an account is given by Dr. Preston (Philos. Trans. N°. 226.), the celebrated anatomist Monf. du Verney traced the eighth and ninth pairs, the medulla spinalis, and the intercostals. The child was well proportioned, the cranium, brain, and cerebellum were wanting; in lieu thereof, remained only a substance, like congealed blood, covered with a membrane.

3. In a case related, and largely commented upon, by the celebrated Wepfer \*, which differs in many respects from other children said to be without brains; the child was well proportioned, its head of the usual size, but its brain had degenerated into vesicles, or hydatides, each of which had its blood vessel (might one from thence infer the natural state of the cortical substance of the brain to be cellular?) and the optic and auditory nerves took their rise from three portions of medullary substance lying upon the sphenoid bone near the sella equina.

\* Manget Biblioth. Auct. Vol. II. p. 339.

4. These singular existences afford useful inferences, and shew that the irritability of the heart, is capable of being sustained, by very low degrees of the nervous power, while that irritability is kept up by the fostering heat of the mother. This feeble life is soon extinguished, when the influences of the mother's warmth and circulation cease (N°. 1). Such infants die as soon as born, or soon after.

5. Such examples more consequentially than experiments demonstrate that the spinal marrow is the principal origin of the intercostal nerves (N°. 2.); and better than ligatures illustrate their vast importance. for,

6. From the plump state of the body, and vigorous appearance of the heart, it is evident the circulation, and the developement of the several organs, had been carried on properly in the fœtus; and that the irritability of the heart derived a sufficiency of nervous influence from the intercostal nerves, and its ganglions, and these again from the spinal marrow, for growth, and that state of existence.

In the essay which was published in the *Philos. Transf.* for 1764, I endeavoured to prove, that as ganglions are seated constantly on the intercostal nerves, and on others sent to muscles whose motions are involuntary, and are very rarely seen on nerves sent to voluntary muscles, and not at all on the sensory nerves; it seems that, by means of ganglions, the motions of the heart and intestines and uvea are rendered uniformly involuntary. I was then, and am still sensible that various strong objections may be made to this doctrine, in common with every other system whatever; but especially every system which pretends



to explain any thing relating to so obscure a part of the animal œconomy, as the nerves, and their faculties. But as this doctrine, weighed against what has been said against it, seems to me to have a considerable preponderance of evidence and probability in its favor, I shall now state the strongest objections which have been opposed to it, and endeavour to answer them.

1. The chief objection which has been made to this doctrine arises from observing, that the spinal nerves, have each one *ganglion*: and that one or two have been observed *sometimes* upon the subdivisions of the fifth pair of nerves.

With respect to the first, it is in the highest degree probable that the *ganglion* observed upon each of the spinal nerves respects solely the intercostals (see Winslow's description of that nerve, Exp. Anat. p. 462.) and is there seated to set apart for the uses of the great sympathetic nerves the *furculi*, which are from each of these ganglions detached to that great pair of nerves. 1. Because the spinal nerves have no other *ganglion* in any part of their course. 2. When nervous twigs are sent off from the spinal nerves, to join others besides the *intercostals*, they have no *ganglions*; as the *nervi accessorii* sent from the upper spinal nerves to join the eighth pair: and the first of the spinal nerves, and the last of the *nervi sacri* are represented by Vieussens\* as being without *ganglions*, and sending no contribution to the intercostals.

Were the *ganglions* only little knots indiscriminately suitable for nerves, which carry the commands of

\* Neurograph.

the will, and those which do not, one might expect them equally frequent on the nerves of the limbs, as at the rise and on the course of the *intercostals*, where they are numerous, large and constant: on the nerves sent to the *sensory organs*, and on the *phrenetic* as well as the *lenticular ganglion* from which the Iris is provided with nerves. In a word the solicitude, so to express myself, and the constancy with which all parts whose motions are involuntary are provided with nerves furnished and beset with *ganglions*; and the great scarcity and rarity of them on nerves detached to muscles subject to our volitions, and the total want of them on the sensory nerves, sufficiently bespeaks their general distinction and use, notwithstanding a few seeming exceptions. I say seeming, because some that are alledged as exceptions are not permanently and constantly found. To ascertain the use and importance of any part of animal structure, we ought to be certain that it is constantly found in that situation; if, on the contrary, it be only accidentally seen, and not perpetually, we can neither assign any important use to it, or draw any important doctrine from it, and have great reason to suspect it to be rather some morbid phenomenon than otherwise. Now this is the case with respect to the *ganglia* described by Mekelius as seated on twigs of the second and third branches of the fifth pair of nerves; betwixt which and other *ganglions* Baron Haller makes a very essential distinction, particularly the *ganglion ophthalmicum*, which he says is constant and perpetual, whereas these beforementio-

ed

ed are not so, for he \* mentions his having examined bodies in which they were wanting.

But, supposing the utmost in favour of these *ganglia* of the fifth pair: the nervous twigs on which they have been observed are chiefly distributed to the salivary and mucous glands, about the tongue, jaw, palate, throat, and nostrils, and therefore may be supposed to have some use in glandular secretion; for we see the glandular parts in the *abdomen* are supplied by the *intercostals* as well as the muscular fibres of the heart and intestines.

2. It has likewise been objected that the *intercostals* send some branches to parts under the controul of the will as the *pharynx* and *diaphragm*; as well as to the heart and intestines, not subject to that controul.

It is well known that the *pharynx* has its most considerable supply of nerves, from the eighth pair: and the *diaphragm* is rendered paralytic by tying or cutting the *phrenic* nerves distributed to it, which shews that its motions have very little if any dependence on the minute filaments, which it receives from the *intercostals*. The motions however of both these parts are properly speaking of the mixed kind, sometimes being *voluntary*, at other times *involuntary*: thus the *diaphragm* moves when we are asleep, as well as when we wake, and continues for some time, even during a profound apoplectic fit: and though we can raise the *pharynx* by an effort of the will, yet in the action of deglutition its motions are chiefly *involuntary* from the *stimulus* of the food, passing down the gullet, as has been shown in the most ingenious

\* Haller, El. Phys. T. IV.

work of a great and worthy man lately deceased, an *essay on the vital and involuntary motions of animals*, by Dr. Whytt \*.

And it deserves to be remarked, that parts, whose motions are of this mixed kind, will be found to have generally a double distribution of nerves, namely such as *are without ganglions*, to subject them to the *will*; and such as *have ganglions* occasionally to support those motions of the same parts which go on without the will; but, these supplies from the *intercostals* being very minute, their action is generally called forth and assisted by some degree of uneasy sensation or stimulus.

If these anatomical objections have not force enough to overthrow our doctrine, the following of a physiological nature, it is presumed, will not be more formidable.

3. It has been objected, that if the *ganglia* intercept the communication between the *sensorium commune* and those parts whose nerves are derived from them, they ought not only to intercept the commands of the will, and render the motion of these parts not voluntary, but they ought also to prevent the impressions made on the nerves of these parts from being conveyed to the *sensorium commune*, i. e. these parts ought to be insensible. The contrary of which is true; for example, the intestines, whose nerves come from *ganglia*, are among the most sensible parts of the body. And if the uneasy sensation in the lungs, in asthmatic cases, was not conveyed to the *sensorium commune*, how could the will redouble the action of the *diaphragm* and the *intercostal* muscles?

\* P.

To this I answer, that the interruption of the parallel direction of the nervous filaments, which probably takes place in *ganglions*, may intercept the efforts of the will, and also render the sensations of parts wholly supplied with nerves from *ganglions*, more indeterminate and confused than in other parts; which in fact is the kind of sensation proper to these parts, yet without rendering such parts totally insensible, which is well illustrated by morbid cases. Paralytic diseases shew that the nerves may be so affected as to become incapable of conveying the commands of the will, and yet remain sufficiently capable of re-conveying sensible perceptions. In the palsies which are most frequent, the parts rendered immovable by the disease have as quick a feeling as those that remain moveable by the will, and, what by the way deserves attention, are often moved involuntarily, especially upon the application of any painful *stimulus*: and it is observable that the paralytic limbs, which are not to be moved by our volitions, are often called into action, when the paralytic person is suddenly thrown into some vehement passion: just as we observe the same cause to produce extraordinary commotions in the heart and intestines, &c. notwithstanding the will, coolly exerted, has no power over these parts.

Various observations shew that the feelings of parts whose nerves come from *ganglia* are by no means acute, but blunt and confused. We have it on the authority of the great Harvey, confirmed by the experiments of Baron Haller, that the heart, though highly irritable, is yet when touched hardly sensible of it. Dr. Haller asserts that the lungs, liver, spleen  
and

and kidneys, all supplied with nerves from the *intercostals*, have been cut in pieces without the animal's seeming to feel pain. And what is less liable to exception, operations and diseases in the kidneys, and ulcers in the lungs, shew their feelings not to be exquisite \*.

The stomach, which has a very large portion of the eight pair of nerves bestowed upon it, by ligatures of this nerve, loses its sensibility and contractive power so perfectly, that the food neither passes down the œsophagus, nor is concocted in the stomach, but, by spontaneous corruption there, puts on the appearance of the fæces themselves in the great intestines †. This proves what was asserted concerning the eighth pair, as being a sensory nerve; and 'tis in consequence of the sensibility which the stomach derives by means of this nerve, as well as its own structure, that the stomach becomes the principal seat of hunger: "And (to use the words of Dr. Whytt ‡), as it is "affected with a more disagreeable sensation, when "we have wanted food for any considerable time, than "the guts, so likewise it is more sensible of an agreeable feeling from grateful food, and in these respects "it may be said to be more sensible than the intestines." Baron Haller has observed that parts which have nerves from *ganglions* are not so distinctly painful as others, "ut anima non adeo accurate locum "dolentem distinguat, sed obiter utcumque, et cum "aliqua latitudine §." And this confused indeter-

\* Haller. Irritab. et Whytt. Path. Essays.

† See Vieussens, Bruni, Morgagni, Haller.

‡ Path. Ess. p. 155.

§ Elem. Phys. T. IV. p. 407.

minate sensation is the sensation proper to the intestines, though in many instances they are the seat of exquisite pain; yet, in consequence of the concurrence and commixture of the nervous filaments in *ganglia*, any painful disease seated in the intestines, or in others of the viscera contained in the *abdomen*, is less determinable to its particular seat, or rather is more apt to affect the parts contained in the *abdomen*, not primarily affected, than diseases of a painful nature, which are seated in the stomach itself, or other parts whose nerves are unsupplied with *ganglions*. And this leads to a natural solution of the cause of that sympathy, that communion of sensation, or imputation of sensation, which so frequently takes place, in the diseases of the contained parts of the abdomen, from which some writers (Linn. \*) have very conclusively argued for the necessity of such a communication of the nervous filaments in *ganglia* as we contend for, from the best anatomical authority, and which appears to have such important uses in the animal œconomy, and to be the occasion of that sympathy or confusion of sensation among the *abdominal viscera* in particular †.

\* In Haller. El. Phys. T. IV. p.

† The solution of the problems concerning the sympathetick affections, or consent of parts, has employed the hands and pens of many ingenious writers; and if all the questions relating to it were discussed, volumes might be filled, and the subject neither exhausted nor understood. The ingenious Dr. Whytt has with great acuteness shewn that sympathy in general is only to be accounted for from a sentient principle, seated in the *sensorium commune*, where all nerves begin, and communicate; his objections to particular sympathies arising from a connexion of nerves in *ganglions* seem inconclusive; for he remarks that such a communication as is supposed in *ganglia* to occasion sympathy,

It

4. It is objected also that every voluntary muscle in the body becomes involuntary when it is strongly stimulated, for example the *acceleratores urinæ* are quite voluntary in their action of expelling the urine, but act involuntarily in expelling the *semen*.

When we consider the state of the soul and body under any great commotion of mind, we find the usual operations of the mind itself are not only interrupted, but those parts of the body too which the will cannot controul are now agitated by the storm; for every one has experienced that the heart and *viscera* in general are vehemently affected by strong passions. The mind is in its turn re-acted upon, by very strong bodily sensations; it being well known that muscular parts, which are ordinarily subject to our volitions, cease to be so if any part is stimulated by exquisitely pleasing, or excessively painful sensations; under such a *stimulus*, they are necessarily contracted or convulsed. But it is not therefore to be concluded that the gentle *stimulus* of the blood on the surface of the heart, and of the air, food, and intestinal juices on the intestines, of which the mind has no conscious perception at all, much less a disagreeable one, can lay it under any similar necessity, as some have argued; and therefore the independence

would cause a confusion in our sensations as well as in the motions of our muscles.—with respect to sensation I have remarked that confusion or indeterminate sensation, is that, which is proper to parts whose nerves arise from *ganglions*—and that the muscular motions of these parts are by means of *ganglions* not regulated by the will, but subsist by the application of an irritating cause: and, instead of proving that sympathy in the *abdominal viscera* does not arise from *ganglions*, they prove that it does. See Whytt on diseases of the nerves.



of these motions on our minds, cannot be explained from this analogy, which does not subsist in fact. This objection therefore can have no force against our doctrine, however it may recoil upon another; as all voluntary muscles whatever may be excited to contractions by irritations excessively painful or pleasing, the contraction in such cases being involuntary, necessary, and uncontrollable. But the *stimuli* that affect the heart and other parts, whose motions are naturally involuntary, are not of this class and strength, and indeed are so little perceived by the mind, as to lay it under no such necessity, as that *stimulus* which renders the action of the *acceleratores urinæ* involuntary in expelling the *semen*.

5. Lastly it has been objected, that, though the motions of the *uvea* are involuntary from light affecting the eye; they are truly voluntary when it contracts in order to the distinct vision of an object placed near the eye, whose minute parts we want to observe accurately.

It seems on the contrary certain, that the contractions of the *uvea*, in order to distinct vision, equally arise from different impressions of light on the *retina*, and are equally involuntary, and solely dependant upon the impressions of different degrees of light in all cases: the distinction indeed is chiefly verbal; as it is granted, that the motions of the *uvea* are involuntary from light affecting the eye, that is, different degrees of light striking upon the *retina* necessarily occasion more or less of contraction in the pupil. But as vision, considered as distinct or indistinct, is occasioned by various impressions of light upon the *retina*, the contraction of the pupil necessarily fol-

lows, according to the degree of that impressi<sup>o</sup>n : in the case of distant objects, the faint impressi<sup>o</sup>ns of light on the *retina* make the pupil contract little, and it remains wide ; for dilatation is the natural state of the pupil. In observing very near objects, the light is *cæteris paribus* stronger, and stimulates the *retina*, and contracts the pupil more.

In a word, the contractions of the *uvea* arise from the sensations of the *retina* involuntarily and uniformly, according to an invariable law and connexion ; otherwise why does the pupil constantly become immoveable, when by a *gutta serena* the *retina* becomes insensible ? Let any one observe the motions of the pupil, by the help of a mirror, they will always find it impracticable to subject them to the will. Indeed it is clear from experiments, as well as diseases, that the *iris*, like all other parts provided with nerves from *ganglia*, has but a dull degree of feeling, and is moved entirely independent of the will \*. “ What persuades me, says M. de Haller, “ that the *iris* is much less sensible than the *retina*, “ is, that if, after having pierced the *cornea*, you irritate or cut the *iris*, it is not therefore contracted, “ whereas the least increase of light makes it contract ; which evidently proves that this contraction “ does not depend upon the proper sensibility of the “ *iris*, but on that of the *retina*. The *gutta serena* “ serves to prove the same thing, the *iris* being no “ ways changed in that disease, any further than it “ is deprived of motion, from the sensation of the “ *retina*, being destroyed by a palsy of the optic “ nerve.” Essay on Irritability, p. 31.

\* See Whytt, Ess. on the Invol. motions. Le Cat, on the Senses. Haller, Elem. Phys. &c.

To conclude, the *ganglia*, respecting their structure, may justly be considered as little brains, or germs of those nerves detached from them, consisting, according to Winslow, of a mixture of cortical and nervous medullary substance, nourished with several small blood vessels\*, in which various nervous filaments are collected, and in them lose their parallel rectilineal direction, according to Baron Haller †, who likewise observes that *ganglia* send off more and larger nerves than came to them ‡; so that a new nervous organization, analogous to the brain, probably takes place in them.

Respecting their uses, *ganglions* are the origins of the nerves, sent to organs, moved involuntarily, and probably the cause, or check, which hinders our volitions from extending to them. As different sources of nervous power, *ganglions* are analogous to the brain in their office, though they derive their nervous filaments (to be new arranged in them), and consequently their power, ultimately from it.

In a word, *ganglions* appear to limit the arbitrary power of the soul in the animal œconomy.

They put it out of our power, by a single volition, to stop the motions of our heart, and in one capricious instant irrevocably to end our lives: and however in the dark we may be, what subordinate agents are substituted, so uniformly to guide and direct, independent of us, our vital and involuntary motions; we must at least clearly discern, in the contrivance, the goodness, boundless, and unerring wisdom, no less than the power, of our adorable Creator! “ad  
“ impellendum fatis, ad docendum parum.”

\* Winslow, *Traité de la Teste*, 629.  
Phyf. T. iv. p. 203.

† Haller, *ibid.*

‡ Halleri Elem.

Received January 14, 1767.

XVI. *Cogitata de COMETIS. Communicated*  
by Benjamin Franklin, LL.D. F. R. S.

Illustrissimæ SOCIETATI REGIÆ,

Ob summum in se collatum honorem,

Cooptationem scil. in eorum Sodalitium,

Hanc qualemcunque Dissertationem,

Ea qua par est observantia,

D. D. C. Q.

Johannes Winthrop,

Apud Cantab. Nov. Ang. Math. & Phil. Prof.  
Hollisianus,

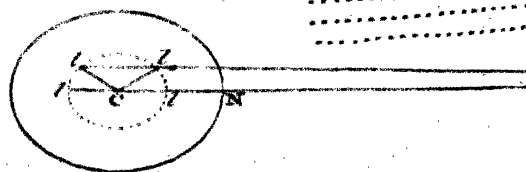
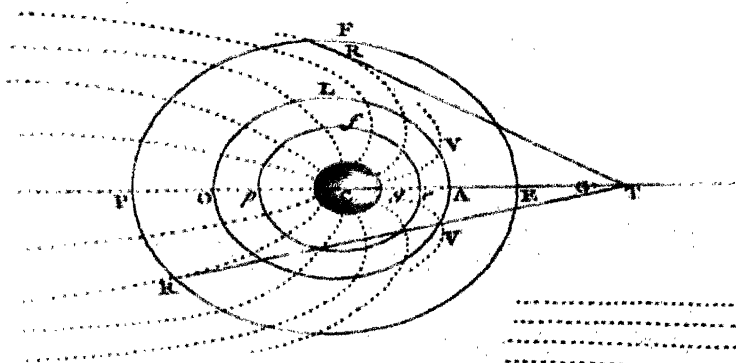
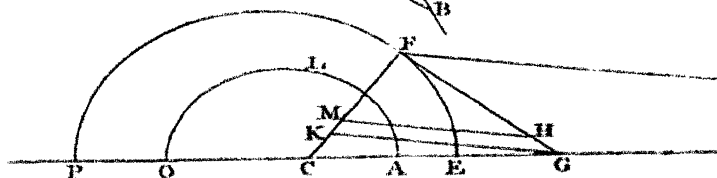
7<sup>o</sup> Maii 1766.

L E M M A.

Read March 19,  
1767.

CUM ex illustrissimi Newtoni in-  
ventis constet, "gravitatem in  
" universa corpora fieri, eamque proportionalem esse  
" quantitati materiæ in singulis, et reciproce propor-  
" tionalem quadrato distantiae inter corporum centra,"  
exinde sequitur, quòd inter bina quævis systematis  
mundani corpora existere potest *limes* attractionis, in

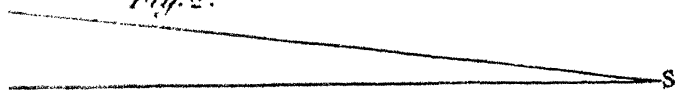




*Fig. 1.*



*Fig. 2.*



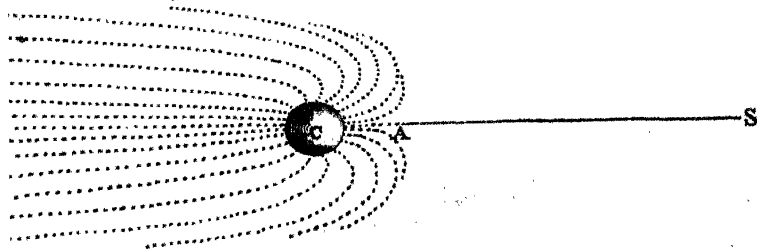
*Fig. 3.*



*Fig. 4.*



*Fig. 5.*



*Fig. 6.*







quo utique situm corpusculum æqualibus viribus utrumque versus urgebitur.

Invenire hunc limitem inter solem et cometas, et quædam inde pendentia, est scopus problematum sequentium.

## P R O B. I.

Datis, materiæ quantitativibus in duobus corporibus, et distantia inter eorum centra; invenire limitem attractionis.

## T A B. VII.

In Fig. 1. sint  $S$  et  $C$  centra corporum, quorum majus,  $S$ ; et quantitates materiæ in ipsis vocentur  $s$  et  $c$  respectivè. Secetur recta  $SC$ , ultra minus corpus  $C$  in infinitum producta, in  $A$  et  $O$ , ita ut sint  $SA$  ad  $AC$ , et  $SO$  ad  $OC$ ; in subduplicata ratione ipsius  $s$  ad  $c$ ; superque diametro  $OA$  describatur semicirculus  $OLA$ : Et limes attractionis erit sphærica superficies circumactu semicirculi  $OLA$  circum axem  $OA$  genita.

Nam ex iis quæ præmissa sunt in lemmate patet, puncta  $A$  et  $O$  esse in limite. Et, si a puncto quovis  $L$  in semicirculo  $OLA$  ducantur rectæ  $LS$ ,  $LC$  ad centra corporum  $S$  et  $C$ , erit, ex natura circuli,  $SL$  ad  $LC$  ut  $SA$  ad  $AC$ , et ut  $SO$  ad  $OC$ ; unde punctum  $L$  est in limite. Pari ratione, omnia puncta in semicirculo  $OLA$ , adeoque in sphærica superficie circumactu istius semicirculi genita, sunt in limite quæsito. Limes igitur attractionis est hæc superficies sphærica, corpori minori  $C$  eccentrica. Q. E. I.

S C H O L.

## S C H O L. I.

Intra hanc superficiem, quam voco *limitantem*, corporis minoris vis plus pollet: Extra, majoris.

COROL. I. Sphæræ limitantis diameter AO, ejusque segmenta AC, CO, sunt ut distantia inter corporum centra.

COROL. 2. Dato quovis puncto in superficie limitante, ut et corporum distantia, datur tota superficies.

COROL. 3. In hac superficie, gravitatio dirigitur ad punctum A, tanquam ad centrum. Ob æqualitatem virium, quibus corpusculum in L trahitur versus corpora S et C, directio vis ex ipsis compositæ bifariam secat angulum SLC; ideoque transit per punctum A; per iii. 3. elem.

COROL. 4. Et ducto perpendiculo CB ad CL, occurrente ipsi LA (productæ, si opus sit) in B; vis ipsa composita erit reciproce ut rectangulum CLB. Nam CD demisso perpendiculo super LA, vis simplex versus C erit ad vim compositam versus A, ut CL ad 2LD, id est, ut BL ad 2CL. Unde, cum vis simplex sit ut  $\frac{1}{CL^q}$ , vis composita erit ut  $\frac{2CL}{CL^q + LB}$ , five ut  $\frac{1}{CL \times LB}$ .

## S C H O L. II.

Si duo corpora fuerint æqualia, limes attractionis esset planum infinitum, distantiam corporum bifariam et ad rectos angulos secans. In hoc casu, CA æquaretur ipsi AS, et punctum O abiret in infinitum.

SCHOL.

S C H O L. III.

Positis, distantia  $SC=d$ ; semidiametro corporis majoris  $S=b$ , et minoris  $C=k$ : si ea fuerit corporum distantia, ut sit  $d:k::\sqrt{s}+\sqrt{c}:\sqrt{c}$ , punctum A continget superficiem corporis C. Idem eveniet puncto O, si, imminutâ paululum distantia, sit  $d:k::\sqrt{s}-\sqrt{c}:\sqrt{c}$ . Sin distantia  $d$  adhuc minor fuerit, Problema evadet impossibile.

P R O B. II.

Idem positis, invenire locum in quo vires corporum sint ad invicem in ratione data.

Sit ratio data  $b$  ad  $c$ , in qua oportet esse vim corporis majoris ad vim minoris. Secetur producta SC (Fig. 2.) in E et P, ita ut sint SE ad EC, et SP ad PC in subduplicata ratione ipsius  $s$  ad  $b$ ; et locus quæsitus erit superficies sphaeræ PFE, diametro PE descriptæ. Q. E. I.

Demonstratur ut Prob. I.

COROL. I. Si secetur CS in G, ita ut sit CG ad GS, ut  $\frac{b \times CE}{c}$  ad ES, punctum G erit centrum ad quod dirigetur composita gravitatio in superficie PFE. Jungantur FS, FG, FC; et agatur recta GK ipsi SF parallela. Cum ratio CG ad GS, five CK ad KF, componatur ex rationibus CK ad KG, (id est, CF ad FS, five CE ad ES) et KG ad KF; et, per constructionem, ratio CG ad GS componitur ex rationibus CE ad ES, et  $b$  ad  $c$ ; consequens est, quod KG

KG est ad KF ut  $b$  ad  $c$ ; id est, ut vis corporis S agens secundum rectam ipsi KG parallelam, ad vim corporis C agentem secundum rectam FK.

COROL. 2. Et, si in diagonali FG sumatur FH = FC, et agatur HM ipsi SF parallela, vis composita in puncto F erit reciproce ut rectangulum CFM.

Demonstratur ut Corol. 4. Prob. I.

Eadem intelligenda sunt de superficie interiori  $pfe$ , et punctis  $g, k, b, m$ , in Fig. 3.

COROL. 3. Ubi  $b$  minor est quam  $c$ , centrum  $g$  versatur intra superficiem  $pfe$ , ut in Fig. 3. Ubi major, centrum  $G$  versatur extra superficiem PFE; eoque longius distabit a corpore C, cæteris manentibus, quo major fuerit ratio data.

Cæterum (ut id obiter moneam), vires conjunctæ gravitatis non in diversis ejusdem superficiiei partibus tantum, sed et in diversis superficiebus, sunt inter se in ratione supradicta. V. gr. Gravitas in puncto F est ad gravitatem in puncto  $f$ , ut rectangulum  $Cfm$  ad rectangulum CFM; in Figg. 2. et 3.

## SCHOL.

Si ratio data eadem sit ac  $s$  ad  $c$ , sphærica superficies PFE in planam mutabitur; haud secus ac in Schol. 2. Prob. I. puncto P in infinitum abeunte. Si ratio fuerit major, punctum P cadet in contrariam partem centri S; et superficies iterum erit sphærica, at corpori majori eccentrica; ejusque diameter invenitur

nitur ut supra. Sin ratio data fuerit major quàm  $\frac{b+d}{b} \times s$  ad  $b^2c$ ; vel minor quàm  $k^2s$  ad  $\frac{k+d}{k} \times c$ ; Problema erit impossibile.

## P R O B. III.

Corpusculorum, conjunctis corporum S et C viribus attractorum, motus generatim describere.

Si corpora S et C medio fluido circumdentur, in quo mergantur corpuscula specificè leviora aut graviora quàm istud medium, corpuscula illa perinde ascendent vel descendent, per utriusque corporis attractionem, ac si ad corpus unicum traherentur; ideoque movebuntur vel in rectis lineis vel curvis, prout eorum motus directi sint vel obliqui, respectu centri compositæ gravitationis. Nam centrum hoc idem valet \* ac corporis unici centrum in eodem puncto locatum.

CAS. I. Corpuscula inter corpora C et S in recta CS sita, quæ specificè leviora sunt medio ambiente, tendunt ad punctum A, Fig. 1. Nam quæ inter corpus C et punctum A sita sunt, ascendent a corpore C; et quæ inter corpus S et idem punctum, a corpore S ascendent, (per Schol. I. Prob. I.) Corpusculum autem in ipsissimo puncto A situm, in æquilibrio detentum, requiescit. Quæ in recta CO sita sunt, ex altera parte corporis C, ascendent ultra limitem O ad altitudinem indefinitam. Hæc enim, in toto itinere, quantumvis longo, ascendent simul ab utroque corpore C et S. Contra fieret, in corpusculis specificè gravioribus: quod et de casu sequente dicendum.

\* Hæc mathematicè dicta sunt, non physice. Nam centra minime trahunt.

CAS. II. Omnia corpuscula leviora, e corpore C oriunda, iis quæ in recta syzygiarum PS fita sunt exceptis, ascendunt in curvilineis semitis, non multum dissimilibus, quantum auguror, eis quæ punctis signantur, in Fig. 4. quarum convexitas obvertitur corpori majori S, et quæ magis magisque tendunt versus plagam ei oppositam. Leviora enim, quæ in superficie limitante OLA fita sunt, ascendunt a puncto A; et quæ in superficie PFE, vel *pfe*, (Figg. 2. et 3.) a puncto G, vel *g*; quoniam hæc puncta sunt centra gravitationis compositæ, ad corpora C et S; per Corol. 3. Prob. I. et Corol. 1. Prob. II. Ejusmodi corpuscula, cum primum expedita sunt a corpore C, ascendunt quaquaversum ab ipsius centro, saltem quam proxime; peragrando autem superficies *pfe*, PFE, &c. ascendunt quasi depulsa a centro *g*, vel G, &c. quod semper jacet inter puncta C et S; et, dum augetur ratio *b* ad *c*, manente distantia CS, perpetuo recedit a corpore C; per Corol. 3. Prob. II. et citius, auctâ quoque distantia CS. Quamdiu intra sphaeram limitantem comprehenduntur, ascendunt fere a corpore C. In transitu enim per superficiem interiorem *pfe*, fugantur a centro *g*, quod inter C et *e* locatur; at, ob compositionem motûs antea acquisiti cum nisu ascensûs a centro *g*, directiones in quibus assurgunt, seu tangentes semitæ suæ, secant rectam Cg in punctis quæ adhuc propiora sunt corpori C quàm est punctum *g*. Egressis extra sphaeram limitantem, et superficies exteriores PFE permeantibus, ascensus eorum magis magisque fit a corpore S. Fugantur nunc a centro G, quod locatur inter E et S; eoque magis appropinquat, cæteris paribus, corpori S, quo altius ascenderunt corpuscula a corpore C.

COROL. 1. Corpuscula, quæ a corpore C prope rectam syzygiarum CS assurgunt, ubi regiones ipsi A vicinas attigerunt, sese ad latera diffundent, cursumque in partes contrarias flectent; velut aqua fontis arte fabricati, simul ac summam consecuta est altitudinem, quaquaversum diffluit, retrorsus jamjam itura. Et omnium quidem semitis corpusculorum ex toto corporis C hemisphærio ipsi S proximo fasciatorum competit vertex V, Fig. 4. seu punctum ex quo curva in contrarium producit. Cis hunc verticem, corpuscula ad corpus S accedunt; trans, ab ipso recedunt. Quæ ab hemisphærio opposito ortum ducunt, ab ipso S nunquam non recedunt.

COROL. 2. Recedentibus corpusculis a corpore S, id est, trans verticem V, angulus RTC, (Fig. 4.) sub semitæ tangente RT et syzygiarum recta CS contentus perpetim minuitur; ad modum parabolæ.

COROL. 3. Vis, quâ corpuscula leviora in his superficiebus sphericis sita ascendunt, eo major est quo propiora sunt illa plagæ oppositionis, pOP. Manentibus enim medii ambientis et corporis immerfi densitatibus, si augeatur vis acceleratrix gravitatis in quacunque ratione, augebitur in eadem ratione differentia gravitatum specificarum, id est, vis qua corpus immersum sursum vel deorsum fertur in isto medio. Augetur autem vis acceleratrix in unaquaque harum superficierum, pergendo a conjunctione CE per F ad oppositionem CP; † per Corol. 4. Prob. I. et Corol. 2. Prob. II.

† Vis acceleratrix augetur in superficie quacunque EFP, ab E ad F et P, quamdiu ratio data in Prob. II. minor fuerit quam  $\sqrt[3]{\frac{5}{4}}$  ad 1, posita  $\frac{5}{4}$ . Si verò hæ rationes æquantur, vires in E et P erunt, non accurate quidem sed quàm proxime, æquales. Id quod ex Corol. 2. Prob. II. facile colligitur.

## P R O B. IV.

Ex supradictis, præcipua caudarum cometicarum phænomena derivare, in theoriâ Newtoni.

Secundùm hunc Philosophum celeberrimum, caudæ cometicæ ad hunc modum formantur: “ \* Caudæ a capitibus oriri et in regiones a sole averfas ascendere, confirmatur ex legibus quas observant. “ † Suspicor ascensum illum ex rarefactione materiæ caudarum oriri. Ascendit fumus in camino impulsu aëris cui innatat. Aër ille per calorem rarefactus ascendit, ob diminutam suam gravitatem specificam, et fumum implicatum rapit secum. “ Quidni cauda cometæ ad eundem modum ascenderit a sole? Nam radii solares non agitant media, quæ permeant, nisi in reflexione et refractione. “ Particulæ reflectentes ea actione calefactæ calefacient auram ætheream cui implicantur. Illa calore sibi communicato rarefiet, et ob diminutam eararitatem gravitatem suam specificam, qua prius tendebat in solem, ascendet et secum rapiet particulas reflectentes, ex quibus cauda componitur.” Hæc est summa theoriæ Newtonianæ: Adjicit autem Auctor, “ Ad ascensum vaporum conducit etiam, quod hi gyrantur circa solem et ea actione conantur a sole recedere, at solis atmosphæra et materia cœlorum vel plane quiescit, vel motu solo quem a solis rotatione acceperit, tardius gyratur. Hæ sunt causæ ascensus caudarum in vicinia solis, ubi orbes curviores sunt, et cometæ intra densiorem et ea ratione graviorem solis atmosphæram consistunt, et caudas quam longissimas mox emittunt.”

\* Newt. Princip. p. 511. Edit. tertiz. † Id. p. 514.



Ex theoria jam exposita, sequentia corollaria levi negotio deducuntur.

COROL. 1. Caudæ cometicæ ad soli oppositum dirigi debent. Cum sol major est quàm cometa quivis, quæ in Problematis superioribus de conjunctis corporum inæqualium viribus earumque effectibus demonstrata sunt, hîc locum obtinent. Corpuscula igitur, ex quavis parte capitis cometæ C excitata, eas, easque solum, attingere debent altitudines, ad quas a viribus ibi agentibus impelli possunt. Quæ versus conjunctionem solis S excitata sunt, attingere possunt sphaeram limitantem juxta A, non autem transgredi; per Cas. I. Prob. III. Aliis ex partibus excitata, altius a capite ascendere possunt, sed eorum semitæ semper detorquentur versus soli oppositum; per Cas. 2. Prob. III. Et in ipso opposito, rectâ ascendunt a capite ad altitudinem indefinitam supra O; per Cas. 1. Prob. III. Præterea, corpuscula ad maximas altitudines assurgunt in ea plaga ubi vis sursum impellens est maxima. Hæc vis autem est maxima in plaga soli opposita; per Corol. 3. Prob. III. Proinde, minima caudarum altitudo (si ita loqui fas sit) spectabit ad solem; et maxima, ad soli oppositum.

COROL. 2. Caudæ, ab extremitate inferiore ad superiorem, dilatari debent. Nam directiones vaporum ascendentium intra quamvis superficiem, PFE, divergunt a punctis inter cometam, C, et centrum compositæ gravitationis, G, jacentibus; quæ puncta eo propius accedunt ad solem, S, quo altius ascenderunt vapores a capite cometæ; ut in Cas. 2. Prob. III. expositum est. Postquam igitur corpusculum transivit ultra semitæ verticem, V, (Fig. 4.) dum a centro fugitivo, G, ascendere conatur, directiones ascensûs su

semper vergent ad parallelismum cum recta syzygiarum, SC, per Corol. 2. Prob. III. nunquam vero attingent.

COROL. 3. Et caudæ longissimæ esse debent in vicinia solis; idque ob sequentes causas. 1. Ob majorem vaporum e cometa deinceps extractorum copiam, majori calori solis quodammodo proportionatam, sive congruentem. Est autem hic calor reciproce in duplicata ratione distantiae a sole. 2. Ob majorem vaporum raritatem, ex eodem caloris gradu pendentem. 3. Ob majorem medii ambientis densitatem. Nam materiam cœlorum, cujuscunque demum ea sit raritatis, densiorem esse prope solem, seu commune centrum gravitatis systematis mundani (nisi quatenus rarefcat ingenti calore juxta solis superficiem) rationi consentaneum videtur. Densiores enim particulae inferiora petunt loca; et inferiores superioribus comprimuntur. Sed in qua ratione distantiae a sole densitas illius medii varietur, id nondum est compertum. Ex hisce duabus causis (2. scil. et 3.) junctim sumptis, oritur major differentia gravitatum specificarum, et inde major vis ascensûs; quæ insuper augetur. 4. Per auctam vim acceleratricem ad solem; ut in Corol. 3. Prob. III. notatum est. Hæc vis est reciproce in duplicata ratione distantiae a sole. 5. Ob diminutam sphaeram limitantem; quo pacto fit, ut complures vapores, qui in majoribus a sole distantibus intra ampliorem sphaeram continebantur, et tunc ascendebant fere a cometa, in minoribus extra contractam seclusi, ascendant potissimum a sole; per Cas. 2. Prob. III. caudamque angustiores efficiunt, at productiorem. Hæc autem diminutio fit fere \* in tri-

\* Non accurate; propter magnitudinem inclusi nuclei.

plicata ratione diminutæ distantiae a sole; per Corol. 1. Prob. I.

Notandum nihilominus, non in ipso perihelio, sed paulo post, caudas fore longissimas. Nam, ob continuationem virium impressarum, effectus solent esse maximi, postquam eorum causæ aliquantum sunt diminutæ. Quemadmodum enim “ \* maxima altitudo  
“ æstus marini non incidit in appulsus luminarium  
“ ad meridianum, ubi vis eorum ad mare elevandum.  
“ maxima est, sed in secundam tertiamve horam po-  
“ stea; pariterque æstas et hiems maxime vigent,  
“ non in ipsis solstitiis, sed quasi triginta diebus  
“ postea.” Sic caudæ cometicæ prolixiores esse debent, postquam cometæ perihelion sunt transgressi, et a sole recedere inceperunt.

COROL. 4. Ex iisdem rationibus concludi quoque potest, quod cometæ caudatissimi erunt ii, cæteris paribus, qui proxime ad solem appropinquant.

Hæc omnia cum phænomenis congruere, notissimum est.

## SCHOL. I.

Si lex vis centrifugæ, ex causis in Corol. 3. enumeratis (aliisque, si quæ sint) oriundæ innotesceret, daretur natura curvarum in Cas. 2. Prob. III. descriptarum, et inde figura caudæ cometicæ *a priori* (ut loquuntur) geometricè determinari posset. Impræsentiarum hoc tantum dicere ausim, quod cauda induet figuram conoëidi cuidam (foran parabolico) non dissimilem, cujus vertex solem respiciet, et axis protendetur in partes a sole aversas. Illius speciem, se-

\* Newt. Princip. p. 424. & 466.

cundum sensa mea, quodammodo adumbrare in Fig. 5. periclitatus fui \*.

Hic verò consideravimus solum motum relativum, quo corpuscula vaporis a capite quiescente in altum assurgerent, et quo calefactæ partes auræ æthereæ, † quæ nullum alium habent motum, reverà assurgunt. Corpusculis autem vaporis omnem retinentibus motum quem derivare possunt a capitis motu, five progressivo circa solem, five circulari circa axem proprium, motus iste cum motu ascensûs componitur; et ex illa compositione orientur incurvatio axis caudæ, et deviatio ab oppositione solis, quæ majores vel minores erunt pro velocitate et directione motûs ab hoc fonte derivati. Et hic obiter animadvertendum occurrit, quòd cum nulla alia cernitur unquam incurvatio aut deviatio, quàm quæ oriri potest a motu progressivo capitis circa solem, verisimile est cometas circa axes non rotari.

## S C H O L. II.

Caudæ a sphaëris planetarum abeuntis pars quædam ad planetas attrahi potest, modò ad aliquem eorum, intra debitam distantiam, accedat. Sed pars major, quantum ego quidem video, caput comitata in regiones a planetis aliisque cometis quàm longissime distans, refrigescet; et condensata, in corpus unde originem traxit, per gravitatem suam paulatim recidet, et illud, tractu temporis, more atmosphæræ undique circumfluet; ita ut, quum exacta periodo ad con-

\* Similem cometæ cujusdam observati imaginem ab Hookio depictam postea deprehendi.

† Vide Addend. I. p. 30.

spectum nostrum redierit, instar coronæ caput æqualiter cingentis videatur, in caudam denuo producenda. Si cometa quivis materiâ caudæ suæ spoliatus fuerit, nonne expectandum foret, ut cauda regenerata singulis revolutionibus contraheretur? Utrum verò res ita se habeat, id utique demonstrandi nulla sese hucusque obtulit occasio.

## P R O B. V.

Invenire limitem attractionis inter solem et datum cometam, ex observatione.

Observetur latitudo capillitii, CA, in Fig. 5. a centro cometæ æstimata, in ea parte quæ caudæ opponitur; et hinc, unâ cum distantia cometæ tam a sole quàm a terra, quæ dantur ex theoria gravitatis Newtoniana, habebitur punctum A, Figg. 1. 4. et 5. vel accurate, vel saltem quam proxime (præsertim si angulus sub rectis a cometa ad solem et terram ductis contentus, sit fere rectus). Inde verò, per Corol. 2. Prob. I. dabitur totus limes. Q. E. I.

Nam punctum A, sive eam partem superficiæ capillitii quæ caudæ opponitur, esse in limite, patet ex iis quæ dicta sunt in Corol. 1. Prob. IV.

## S C H O L.

In hunc finem, necesse est cometam a sole intra certos limites distare. In descensu ab aphelio, tam prope ad solem accedere oportet, ut vaporis incalcentis columna assurgere incipiat, et atmosphæræ forma rotunda mutari in oblongam. Hoc prius fieri nequit, quàm CA (Fig. 1.) minor sit semidiametro  
Vol. LVII.
U
atmos-

atmosphæræ. In majoribus distantiiis, cometa capillitio ornatur, caudâ verò destituitur; ut in Schol. 2. Prob. IV. Sin plus æquo appropinquet, coma a fronte capitis retro versa et quasi abrafa\*, tota in caudam abibit. Sed hoc perpaucis cometis, iisque non nisi per breve tempus, evenire potest. In utroque casu, nullus observationibus hîc postulatis relinquitur locus. Eadem dicenda sunt de ascensu cometæ a perihelio. Limites autem ex hac parte axis transversî orbitæ majori intervallo ab invicem distabant quàm ex altera. Ubi cometa prope solis oppositionem versatur, hujusmodi observationes peragi nequeunt.

COROL. 1. Latitudo capillitii, in ea parte quæ respicit solem, quamdiu cometa intra limites in Scholio præcedente memoratos versatur, est ut distantia cometæ a sole; per Corol. 1. Prob. I.

Et hoc cum phænomenis consentire, auctorem habemus Newtonum, observationibus Hevelii fretum. " Atmosphæræ cometarum, ait ille †, in descensu  
 " eorum in solem excurrente in caudas, diminuun-  
 " tur, et (ea certe in parte quæ solem respicit) an-  
 " gustiores redduntur; et vicissim, in recessu eorum a  
 " sole, ubi jam minus excurrunt in caudas, amplian-  
 " tur; Si modo phænomena eorum Hevelius recte  
 " notavit."

COROL. 2. Hinc innotescit quantitas materiæ in cometa. Nam cum dentur SC et CA, datur earum differentia SA; et inde datur ratio SA ad AC; ut et ratio hujus duplicata, quæ (per solutionem Prob. I.)

\* Vide Schol. 3. Prob. I. et confer observata Hevelii de cometa anni 1665, die 20 Aprilis; infra citata in p. 148.

† Newt. Princip. p. 516.

eadem est ac ratio materiæ in sole ad materiam in cometa.

COROL. 3. Hinc, et ex observata diametro, innotescit etiam densitas cometæ. Nam densitas sphaeræ est ut ejus materia directe, et cubus diametri inverse.

Newtonus ex fecundo gravitatis principio elicuit methodos determinandi densitates solis, lunæ et planetarum satellitio stipitorum. Ex eodem fonte derivari posse cometarum quoque densitates, ostendere jam conatus sum. En ! specimen calculi, hæctenus, quod sciam, intacti \*; quod curiosis naturæ scrutatoribus haud ingratum fore spero.

In exemplum esto cometa anni 1665, de quo Hevelius in Cometogr. p. 898. hæc scribit. " Die 8 Aprilis, St. N. mane, magnitudinem capitis ex maculis lunaribus inveni; nucleum interiorem cum tota materia adhærente, sive crinibus circumfusus, æqualem esse toti insulæ Siciliæ, hoc est, summum 6'; nucleum vero solum haud majorem esse insulæ Corfica lunari, hoc est,  $\frac{5}{8}$  part. unius digiti lunaris, sive 12'' vel 13''." Cometa tunc erat in 23° 29' ♄, cum 25° 49' Lat. Bor. Invenio jam, per tabulas Halleianas, distantiam cometæ a sole (SC) 62735; et a terra, 58441; talium partium qualium

\* Ex ingenti caloris gradu quem terra arida apud cometam anni 1680 in perihelio versantem ex radiis solaribus concipere posset, qui, Newtoni computo, quasi 2000 vicibus major erat quam calor ferri candentis, concludit Auctor celeberrimus, quod corpora cometarum sunt solida, compacta, fixa ac durabilia ad instar corporum planetarum. Nam si nihil aliud essent quam vapores vel exhalationes terræ, solis et planetarum, cometa hicce in transitu suo per viciniam solis tanto calore itatim dissipari debuisset." Princip. p. 508. Præter hoc, nihil de cometarum densitatibus vulgatum invenio.

media telluris a sole distantia sit 100,000: et ponendo CA, latitudinem capillitii versus solem, æqualem dimidio capitis, five 3', ea erat 51; et AS, 62684; et semidiameter nuclei 1,7 earundem partium. Proinde, materia in sole est ad materiam in cometa, ut  $62684 \times 62684$  ad  $51 \times 51$ , five ut 1 ad  $\frac{1}{3310724}$ . Porro, posita solis parallaxi media 8'', 68 (ut ex nupero veneris sub sole transitu collegit vir rerum astronomicorum peritissimus Jacobus Short, S. R. S.) semidiameter terræ est 4,2; ejusque materia  $\frac{1}{34286}$  pars materiæ solis; ut meus profert calculus. Quocirca, densitas terræ est ad densitatem cometæ, ut

$\frac{1}{4,2 \text{ cub.} \times 34286}$  ad  $\frac{1}{1,7 \text{ cub.} \times 1510724}$ ; id est, ut 1 ad 3,44. Hic igitur cometa, qui in perihelio suo quasi decuplo propius quam terra ad solem accedebat,  $3\frac{1}{2}$  fere vicibus densitate terram superabat.

Veruntamen, hos nolim venditare numeros tanquam perfectos, et cometæ hujus densitatem absolute exhibentes. Instituto meo sufficit, si, dum calculi methodum illustrent, non longe a vero aberrant; summam enim accurationem ab ipsa Hevelii observatione haud esse petendam, palam est. Probabile omnino videtur, quod observator illustris magnitudinem capitis æstimârit secundum rectam axi caudæ normalem,, quippe in ea sola directione dimetiri potuit caput absque cauda; et capillitii latitudo, de qua hîc agitur, nempe versus solem, paulo minor fuerit quam dimidium istius magnitudinis. Hæc suppositio non male quadrat cum ultima observatione hujus cometæ, quam Hevelius habuit die 20<sup>o</sup> Aprilis. Eo tempore distantia cometæ a sole, meo computo, erat 24237; et a terra, 89602. Jam, apparens latitudo capil-



capillitii versus solem est ut distantia cometæ a sole directe, per Corol. 1. Prob. V. et ut distantia a terra inverse; id est, ut  $\frac{6}{3} \frac{2}{8} \frac{7}{4} \frac{3}{4} \frac{5}{1}$  ad  $\frac{2}{8} \frac{4}{9} \frac{2}{6} \frac{3}{5} \frac{7}{2}$ , five ut 4 ad 1. Quare, si in priore observatione fuisset 3', in posteriore esset 45''; et capillitium emeretur super nucleum 40'' circiter; quod quidem non adeo ægre, adhibito perspicillo, perceptu foret. At testatur Hevelius, quod "die 20<sup>o</sup> Aprilis, cum a nobis ultimum obser-  
"varetur cometa, in frontispicio capitis materia illa  
"dilutior jam adeo erat contracta, attenuata et diffi-  
"pata, ut parum admodum amplius superesset; ad  
"utrumque latus vero satis dilatata extitit." Credibile igitur est, latitudinem anteriorem capillitii, etiam in prima observatione, minorem extitisse quam lateralem; i. e. minorem quam 3'. Quod si hæc latitudo minor fuerit quam 3', minuenda erit materia cometæ, ejusque densitas, in duplicata ratione, quam proxime; per Corol. 2. et 3. Prob. V. Hujus ergo cometæ densitas non major est quam quæ supra definita est; sed potest esse aliquanto minor.

Quærebam itidem densitatem cometæ anni 1682. "Aug. 20. St. V. diameter capillitii axi caudæ per  
"nucleum normalis, mensurante Flamstedio, erat  
"2' 0''; cujus distantia ipse nucleus vix  $\frac{1}{10}$  æquabat;  
"ideoque latus erat circa 12''." Hic iterum necesse habemus sumere dimidium diametri capillitii pro latitudine ejus versus solem; quanquam vix dubium est quin hæc aliquantulo minor fuerit illo. Et in hac suppositione, iisdem calculi vestigiis insistendo, inveni densitatem terræ esse ad densitatem cometæ ut 1 ad 0, 4562; seu ut 11 ad 5 circiter.

Notatu non indignam arbitror aliam hujus cometæ observationem a Flamstedio factam die 4 Septemb.

quo tempore erat “nucleus limbo capillitii vicinior  
 “quam antea [20 Aug.] duplo fere.” Supra mon-  
 stratum est, quod capillitii latitudo apparens versus  
 solem est ut distantia a sole directe, et a terra inverse;  
 ex quo, computum incundo, invenio capillitii latitu-  
 dinem die 4 Septemb. esse ad ipsius latitudinem 20  
 Aug. ut 1 ad 2, 1542. Quod cum observatis probe  
 congruit; ut conferenti liquebit. Fatendum tamen,  
 maximam hujus differentiae partem secundae illarum  
 rationum deberi.

Vellem sane plura adducere exempla, cæterosque  
 cometas, ratione densitatis, inter se conferre; sed ob-  
 servationum penuria impedimento fuit quo minus  
 aliorum densitates ad calculum revocaverim; nullæ  
 enim extant, quas mihi quidem videre contigit, adeo  
 subtiles ut huic disquisitioni apte inservire queant.  
 Nec mirum; quum admodum difficile est, arcus tam  
 exiguos ad amissum mentiri. Quantum ex allatis  
 conjectura assequi possum, eadem hîc valet regula  
 quam obtinere demonstravit Newtonus in planetis,  
 “eos nempe densiores esse, cæteris paribus, qui sunt  
 “foli propiores\*.” Insuper colligere videor, quod  
 sicut cometæ ut plurimum ejusdem fere sunt magni-  
 tudinis cum planetis, ita non multo plus discrepant  
 cometæ a planetis quoad densitatem quam planetæ  
 dissident inter se. Nam terra, per Newtoni demon-  
 strata, sextuplo densior est quam saturnus. Quin-  
 etiam, quod cum distantiae periheliæ sunt æquales,  
 cometæ planetis rariores sunt; — in hunc fortasse fi-

\* Secundum superiorem calculum, cometa anni 1665 erat  
 quasi  $7\frac{1}{2}$  vicibus densior quam cometa anni 1682. Eorum au-  
 tem distantiae periheliæ, Hallesio computante, erant 10649, et  
 58328.

nem, ut in transitu prope planetas, minus turbent eorum motus; et ut, eidem gradui caloris objecti, facilius vapores emittant, ad formandas caudas.

Sed manum de tabula; neque enim conjectandi licentiæ indulgendum. Si in posterum astronomi observationes hujusmodi instituerint, ea sedulitate qua phænomenis stellarum tam fixarum quam errantium invigilare solent, certiores tandem penitioresque siderum caudatorum cognitionem sperare licebit.

### P. S.

EST et infra superficiem cometæ limes attractionis, intra quem vis solis fortior est quam cometæ. Hujus investigatio pendet a diversa gravitatis lege, crescentis scil. in simplici ratione distantiae a centro; per Prop. 73. Lib. I. Newt. Princip. ejusque natura est, quod si a quovis ipsius puncto  $I$  (Fig. 6.) ducantur rectæ ad centrum solis  $S$  et cometæ  $C$ , quadratum prioris in posteriorem ductum,  $SI \times IC$ , efficiet solidum datum; quod eandem quidem habebit rationem ad cubum semidiametri cometæ,  $CN$ , quam habet materia in sole ad materiam in cometa. Verum, cum hujus limitis diameter perexigua est respectu distantiae a sole, et propterea omnes rectæ  $SI$ , ideoque et  $IC$ , sunt quam proxime æquales, hæc superficies  $IIII$  parum differet a sphærica; ejusque diameter erit reciproce ut quadratum distantiae cometæ a sole. Sed hæc, aliaque ad hunc casum spectantia, Prob. II. consimilia, mathematicæ duntaxat sunt contemplationis; eoque consulto in superioribus prætermisimus, utpote ab argumento nostro aliena.

ADDENDA.

## A D D E N D A.

## I.

Afcenſum caudæ cometicæ, quem ope cujuſdem medii cœleſtis perſpicaciſſimus Newtonus explicatum dedit, aliquantulum illuſtrare aggreſſi ſumus. Quænam verò ſit iſtius medii natura, quiſve terminus, magna quæſtio eſt, et tenebris involuta. Diverſis nominibus illud Newtonus inſignivit, ſcilicet auræ æthereæ, materiæ cœlorum, et atmophæræ\*. Aliis in locis ſummus philoſophus aſſeruit, et variis iis que graviffimis rationibus aſſertum ſtabilivit, “cœlos reſiſtentia deſtitui”; quo poſito, hæc aura ætherea non poteſt eſſe non rariffima; et materia cœlorum nihil aliud quam vapores longe tenuiffimi. Corpus ſolis ingenti atmophæræ pondere premi, non deſunt argumenta quæ nobis fidem faciant; et ex macularum ſolarium phænomenis certum eſt, eam, unâ cum corpore ſolis, circa axem ſpatio  $25\frac{1}{2}$  dierum rotari. Quòd ſi hæ maculæ triduo diutius pone ſolem latent quam ſeſe nobis conſpiciendos præbent, ut a quibuſdam obſervatum eſt, neceſſe eſt altæ ſint ſupra ſolis ſuperficiem parte  $\frac{1}{3}$  diametri ſolaris; et ad hanc minimùm altitudinem porrigi ſolis atmof-

\* Vide periochen citatam ſub Prob. IV. in qua Auſtor has voces promiſcue uſurpâſſe videtur; ſaltem non præciſe inter eas diſtinxit.

† Wolfius in Aſtron. macularum ſolarium altitudinem multo majorem juſto æſtimavit, ſcil.  $\frac{3}{4}$  diametri ſolis: maniſeſto errore; ponendo ſinum verſum 10 graduum æqualem 15 centeſimis partibus radii, cùm revera ſit æqualis tantùm 15 milleſimis.

phæram, par est credere. Cometa anni 1680 in perihelio minus distabat a sole quàm parte sexta diametri solis; et in vicinia illa, ex auctoris clarissimi sententia \*, resistantiam nonnullam sentire debuit. Sed quæ sit illius atmosphæræ altitudo, hucusque incognitum est. Verisimile videtur, eam intra modicos terminos contineri. Etenim, si ad orbem planetarum pertingeret, caudæ cometicæ per eos transeuntes, impulsu illius gyrantis tanquam vento validissimo raptæ, ab occasu in ortum detorquerentur. Atqui medium, de quo quæstio est, eousque extendi oportet, quo cauda cometæ cujuscvis pullulare inceptit. Hic locus orbibus mercurii et veneris, immo et ipsius terræ †, superior est. Tam immanis autem amplitudo atmosphæræ solis concedi nequit.

Quid de re tam obscura statuendum sit, incertus hæreo; hæc tantum rata habens, medium hocce tenuissimum esse, et quam facillime rarefcere; paulo autem densius esse prope solem quam ulterius, terminumque ejus extra sphæram orbis magni esse locandum; item, non modo non rotatum vertigine quali corpus solis, sed et summè quietum esse ac tranquillum. Denique, non abfimile videtur, caudarum materiam longe magis volatilem esse quàm exhalationes e corporibus quibuscumque terrenis aut planetariis elicitas.

## II.

Quibus usibus inserviant cometarum caudæ, id est alia quæstio, explicatu difficillima. Censuit Newto-

\* Princip. p. 525.

† Constat ex phænomenis cometæ anni 1680. Alii cometæ hunc terminum ad orbem usque martis, aut supra, forsan amoveant. Sed hoc non exploravi.

us “ eas \* ad conservationem marium et humorum  
 “ in planetis requiri, ex quibus in planetas attractis  
 “ et cum eorum atmosphæris mixtis, quicquid li-  
 “ quoris per vegetationem et putrefactionem consu-  
 “ mitur, et in terram aridam convertitur, continuo  
 “ suppleri et refici possit.” Verùm cùm, ipso judi-  
 ce †, “ perexigua quantitas aëris et vaporum ad om-  
 “ nia caudarum, etiam si spatia immensa occupantium,  
 “ phænomena sufficiat”; et cùm hujus quantitatis  
 perexiguæ perexigua tantum pars in unumquem-  
 que planetam incidere queat (ni mea fallat opinio in  
 Schol. 2. Prob. IV. prolata), ambigi potest an hic sit  
 e præcipuis cometarum finibus. Sed nihil statuo.  
 Aliorum esto judicium.

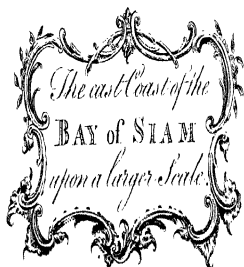
\* Princip. p. 515.

† Id. p. 513.



WEST

PART OF THE  
BAY OF SIAM



Bottom of the  
Gulf of Siam  
River Merga  
Canton City

Cape Nofium  
non Siam

The Bay of  
Wild Beasts

Some days sailing South from the Bottom of the Gulf of Siam

The Difficulty of doubling Cape Siam in sailing out of the bay appears to have been the reason of the inconsistent information which Ptolemy received, & the Chief Cause of his Changing his opinion concerning a

NORTH

proper allowance for the Word Semi. But both reports may be easily reconciled by supposing, that some Ships might double it very soon & so reach Calligara in a few days whereas others by having more unfavorable winds might not be able to accomplish the same distance in a great many days.

The Bay of the Sines

Calligara Situated upon the River Cotarim M. Heracleot.

Also to the South

Unknown Marshy Country to the east of it.

River Merga

οιτο ταυτας ομοδοειν ανατολικωτεραν αστην ειναι των κατιγαραν. T. G. L. c. 15.

Cambodia City situated 3 deg<sup>s</sup> to the east of Calligara or 3 deg<sup>s</sup> of distance from it.





Philo's Trans. Vol. LXXI TAB. IX. p. 455.

WEST

Part of the Island of BORNEO  
one of the Islands of SATUM

MALACCA

MAGNUS SEVEN PTO  
OR BAY OF SIAM

Calligara or  
Pantonaga

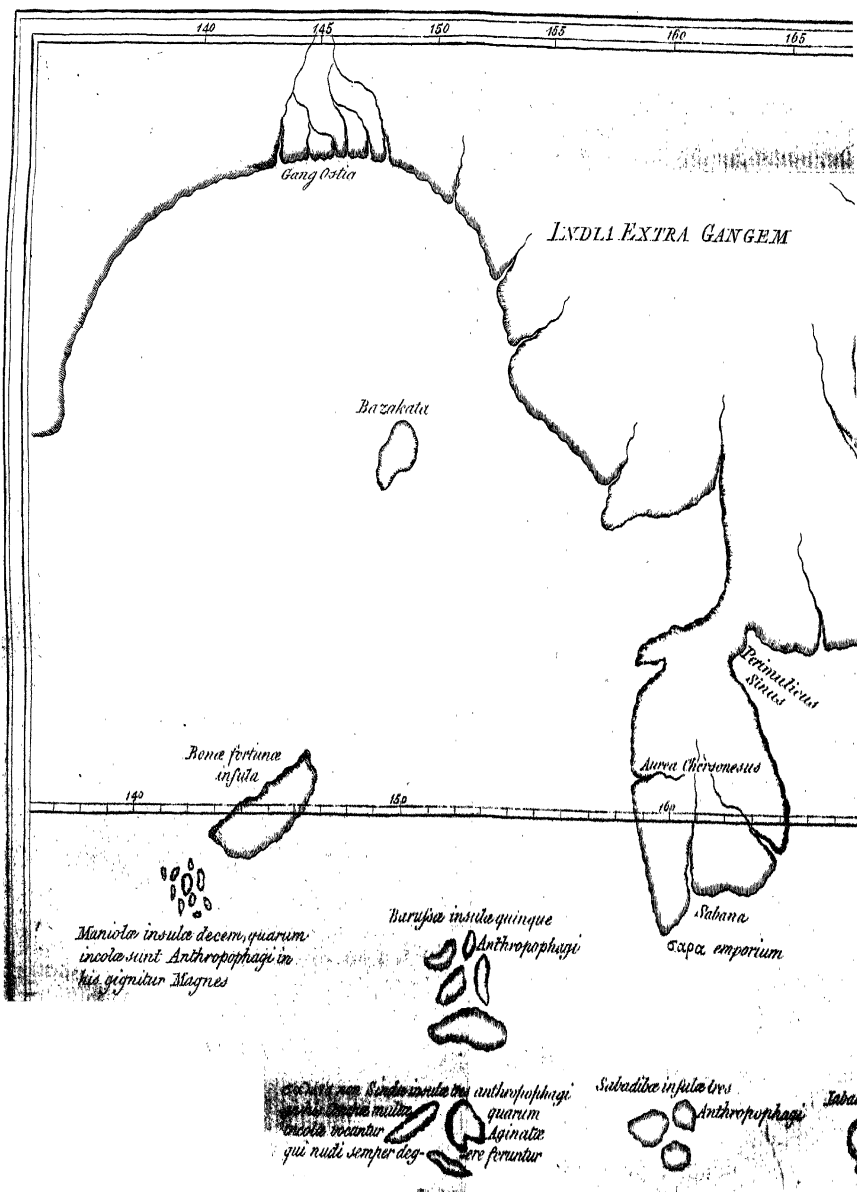
Cambodia City

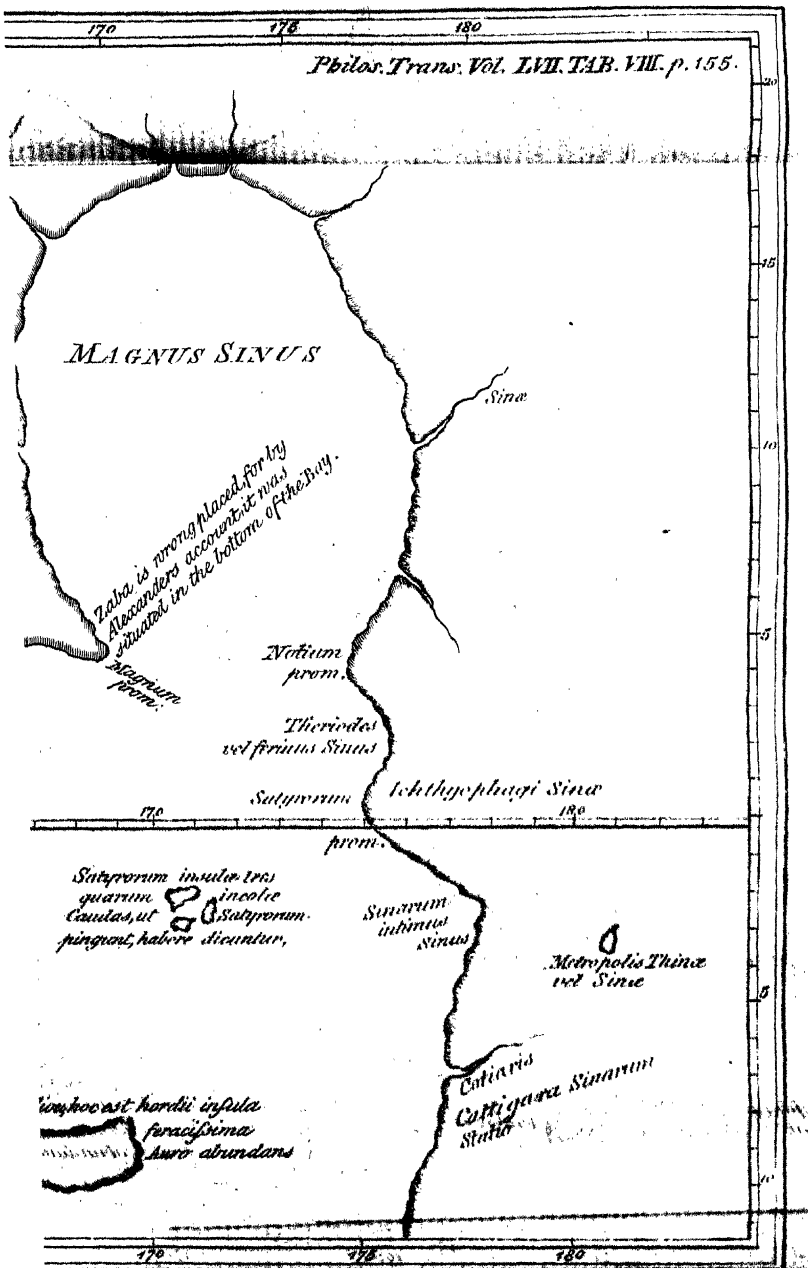
The unknown Island to the east of  
the Metropolis Siam

Part of  
COCHIN CHINA

NORTH









Received February 6, 1767.

XVII. *Some Attempts to ascertain the utmost Extent of the Knowledge of the Ancients in the East Indies: by Mr. John Caverhill.*

Read March 19,  
1767.

**A**L L the moderns who have treated of the geography of the ancients, more particularly in their account of the Sinæ, the most oriental country they were acquainted with, have differed so widely from the only guides they had to follow to the east of the Ganges, that I have undertaken to lay before the Royal society some observations, which may contribute to determine, with greater precision, the situation and limits of this country.

But as geographers have been unanimous in supposing the Aurea Chersonesus to have been the same as the present kingdom of Malacca; the only difficulty, which remains, is to discover how far they sailed beyond it, in the days of Ptolemy. And, that the proofs alledged in support of what may be asserted in the course of this disquisition may be better understood; I have thought it necessary to annex a correct modern chart of this Peninsula, and have also added the outlines of Ptolemy's, (See Tab. VIII. IX. X.) for reasons which will soon appear.

It may be proper first to observe, that Cattigara was the name of a port situated somewhere beyond

the Aurea Cherfonefus or Malacca; and that the antients had never sailed farther than Cattigara: for contiguous to it was a terra incognita<sup>a</sup>. But at what distance the Aurea Cherfonefus was from Cattigara, Ptolemy himself was ignorant; for he says, “that Marinus, who is quoted by him upon this occasion, had not marked the number of the stadia: *τε δε απο της χρυσης Χερσονησος επι τα Καττιγαρη διαπλε πν σταδιασμον ο Μαρινους ουκ εκλιθεται*<sup>b</sup>.” It was said however that Alexander reported the land from thence lay upon the north side of the line; and that those who sailed along the shore, arrived at the city of Zaba in twenty days; from Zaba sailing south, and especially to the left hand, in some days they reached Cattigara: *φησι δε Αλεξανδρον αναγεγραφεναι την γην εντευθεν εναντιαν ειναι τη μεσημερια, και τους πλεονας παρ αυτην εν ημεραις εικοσι καταλαμβανειν πολιν Ζαβας. απο δε των Ζαβων προς νοτου διαπλευσαντας, και μαλλον εις τα ευωνυμα ημερας τινας, εκδεχεσθαι τα Καττιγαρη.*” In order therefore to endeavour to acquire some knowledge of the distance, Ptolemy begins by comparing it with the same number of days sailing on a part of the coast of Africa, with which he was acquainted; and, after this uncertain calculation, concludes, that these two places might be distant from each other 17 degrees and  $\frac{1}{6}$ .

As his only view through this whole chapter is to finish what he had begun in the foregoing ones, the longitude of the then known world, his errors are the more excusable; especially as his calcula-

<sup>a</sup> Ptol. Geograph. lib. i. c. 17. Paris, 1546.

<sup>b</sup> Lib. i. c. 14.



tions will be found very near the truth: his greatest mistake therefore is in adding these 17 degrees to the longitude of the earth; whereas Cattigara was to the latitude of Malacca, and almost upon the same Meridian.

Although Ptolemy no where particularly mentions from what part of Malacca the sailors who went this voyage took their departure, yet as his most southern point of land in this Peninsula, is exactly 17 degrees of longitude from Cattigara, and as Romana is the most southern cape of Malacca at present, this clearly indicates it to have been near that cape, but to the west of it.

For elsewhere he acquaints us<sup>c</sup>, “ that he had  
 “ learned many particulars concerning the inland  
 “ parts of the provinces and countries of India, even  
 “ to the Aurea Chersonesus, and from thence to Cat-  
 “ tigara, and the sailors who went this voyage agreed  
 “ that it was to the east; but that they immediately  
 “ returned towards the west; and that the time  
 “ for performing this last voyage was irregular, and  
 “ not fixed: *παρ ὧν και τα τε αλλα περι την Ινδικην*  
 “ *μερικωτερον, και καλα τας επαρχιας, εμαθομεν και ταυτας*  
 “ *της χωρης ενδηερων, μεχρι της χρυσης Χερσονησος, και εν-*  
 “ *τευθεν εως των Κατιγαραν. το μεν οτι προς ανατολας εστιν,*  
 “ *ο πολλους εσπλευοντων, και παλιν εξισυντων προς δυσμας συν-*  
 “ *ισορουσιν. την δε ατακτον και ανωμαλον τον χρονον των δια-*  
 “ *νυστων προσομολογησιν.*”

To explain this passage it may be observed, that their sailing first to the east was from necessity, because they had not as yet doubled cape Romana, the

<sup>c</sup> Lib. i. c. 17.

most southern point of land in Asia, from whence they were obliged, by their own rules of sailing, to return towards the west, after having doubled the cape. This does not appear so westerly by the chart as it did to them; because a strong easterly tide sets by the cape, which, by carrying them too far east, would make their next course toward the west, and which would soon become intirely so by the figure of the land <sup>a</sup>.

Alexander reported, that the land lay north of the line; they had imagined cape Romana, as well as some other parts of Malacca, lay south of it; they were therefore, no doubt, surprized to find the land retreat.

He has told us that they arrived at Zaba in 20 days, and from it sailing south, but rather to the left hand, in some days they reached Cattigara. Zaba therefore stood somewhere upon the bottom of the bay of Siam; because no other situation but the bottom of the bay of Siam obliged them to alter their course to the south; and including the inequality of the coast, it will be allowing a sufficient distance for the number of days they were in sailing it.

From Zaba they began to sail south, and immediately follows, rather to the left hand; now they could not sail south and incline to the left hand without approaching the east; this therefore might be any point between the south and east. The figure of the coast runs exactly in this direction, and is a proof of the accuracy of the description.

<sup>a</sup> Nicholson's Observ. upon East India Voyages.

In this course they continued some days before they reached Cattigara. Ptolemy justly finds fault with the uncertainty of the expression, *some* days, and is at a loss how many days to allow for *some*; but, as he had no better information, and as they arrived at Cattigara by observing this south east course, it must have stood somewhere to the north west of the mouth of the river Cambodia.

Marcianus Heracleota also expressly mentions that they which sailed to Cattigara held the same course. His words are, "sailing therefore out of the great bay, and the promontory Notium, as if to the south, and keeping the country of Sinæ to the left and to the east; you meet with a bay pertaining to wild beasts, which reaches to the promontory of Satyrs; that after this promontory follows the greatest bay called the bay of Sinæ, which extends to the Terra Australis et Orientalis incognita, into which bay runs the river Cotiarin, and upon which river stands Cattigara a sea-port of the Sinæ: Πλοῦσι τινὲς λέγειν ὅτι μὴ μόνον κολπὸν, καὶ τὸ Νοτίου ἀκρὸν, εἰς ὅρας τὴν μισσημῆραν, καὶ ἀρμόττειν ἐχέει τὴν τε τῶν Σινῶν γῆν, καὶ τὴν ἀνατολὴν· ἐκδεχέαι κολπὸς θηελωδῆς καλουμένος, διήκων μέχρι τοῦ ἀκρωτηρίου τοῦ καλουμένου Σατύρων ἀκροῦ. Ἀπὸ δὲ τοῦ ἀκροῦ τῶν Σατύρων, ἐκδεχέαι κολπὸς μεγίστος, καλούμενος Σινῶν κολπός. παρῆκει δὲ ἄτος μέχρι τῆς μισσημῆρας αἰνῶτος γῆς, ἣ συναπτεῖ καὶ τῆς ἀνατολῆς αἰνῶτος γῆς. ἀπὸ δὲ τοῦ Κολιάρου ποταμοῦ, ἐκδοτὶ χεῖται Καττιγάρα ἄρος μὲν Σινῶν."

For these reasons it is extremely probable that Cattigara stood somewhere upon the north east coast of the bay of Siām. A very intelligent modern

modern Navigator\* gives the following description of it: "For 50 leagues from Liam point it is a barren sandy desert to Ponteamas, by far the most considerable port on the coast, and a place of pretty good trade for many years, but a narrow river, which in the rainy seasons of the south west Monsoons has communication with Banfac or Cambodia river, which made it draw foreign commerce from the city of Cambodia hither, for the city lieth near 100 miles up the river, and most part of the way. a continual stream, made the navigation so troublesome as few cared to trade to it, for which reason they came to Ponteamas."

As this therefore is by far the most considerable port on the coast, we may reasonably suppose it to be the same with the antient Cattigara. The following arguments will serve to confirm the validity of this supposition.

The distance between Ponteamas and the bottom of the bay, where they began sailing for some days south, may very well agree with the 4 degrees Ptolemy allows for it. It may be remembered that he had supposed the 17 degrees beyond Malacca, were situated to the east of it; this supposition which has before been found to be erroneous, Ptolemy seems now to reason from as a fact, in order to support his former hypothesis, notwithstanding his declaration that he thought it ridiculous to imagine, as others had, that *τινὰς ἡμέρας* should signify many days. We have already seen that his first error consisted in imagining that Cattigara lay 17 degrees east

\* Hamilton's Account of the East Indies, vol. ii.

of cape Romana, whereas it was nearly to the west of it. The second error was in protracting the east side of the bay of Siam 18 degrees farther than the land at present exists. And it will immediately appear in what manner this happened.

The boundaries of the Sinæ to the west, is in his 173 degree of longitude, and the most southern end of that parallel, in the 17th of north latitude<sup>f</sup>. Cattigara in the 177th and 8th of south latitude<sup>g</sup>. This difference was the whole maritime extent of the country. In his description of the west side of the bay, he had consumed 13 degrees of longitude, that is from cape Romana to the bottom where the land began to reflect south; and these are what he allowed for the 20 days sailing already mentioned; there were only 4 degrees remaining between the bottom of the bay and Cattigara; but, as that would not at all agree with the accounts he had received of its being a great bay, when one side was so much shorter than the other, he might think his first explanation of the word *some*, erroneous; more especially as the other signification that was given, which was to take it for *many*, even so *many*, that they could not be numbered, would intirely coincide with his first

<sup>f</sup> Lib. vii. cap. 2.

<sup>g</sup> Lib. vii. cap. 3. It has been a prevailing opinion that Agathemerus made these tables we have annexed to Ptolemy's geography, but this appears only to have been done in consequence of the longitudes and latitudes Ptolemy had settled. For when we see him fall into so great an error in his first book, which none doubts to be his own, the adding these supposititious 17 degrees to the longitude of the earth; why may we not with as much appearance of justice lay this second fault to his charge, especially as we have no proofs to the contrary?

Theory. Accordingly he has put down 25 degrees of latitude for *some*; but in such a manner that the south end of this imaginary line was not more than 4 degrees east of the north end in the bottom of the bay of Siam; by which he has not only contradicted that very Alexander he himself quotes, who told him the land was to the north of the line; but renounced his first rational opinion, in thinking four degrees might be a sufficient allowance for *τινας*.

2d. The country beyond Ponteamas exactly agrees with Ptolemy's description of that beyond Cattigara, "a marshy country, which produced reeds of such a size, that when they were joined and tied together, they were enabled to pass from one side to the other. και τα αναβολικώτερα ταύτων, αίνωτος ἐς γῆ, λιμνας ἐχουσα ἰλευθεις, ἐν αἷς καλάμοι μεγάλοι φύονται, και συνεχεις οὕτως, ὥς τε ἐχόμενους αὐτῶν, ποιεῖσθαι τὰς διαπεραιώσεις <sup>h</sup>." Now the kingdom of Cambodia is annually overflowed with water during the south west monsoons, the very season in which the ships arrived there; so must have exactly answered these appearances and his description.

"Cambodia, or Camboxa, is annually overflowed by the river Menam, one of the largest in India, carrying so much water that it floods and covers the fields, like the Nile in Ægypt; for six months it runs backwards. The reason of it is the extent and plainness of the country it runs along, and the southern breezes which choak up the bar with sand <sup>i</sup>."

<sup>h</sup> Lib. i. c. 17.

<sup>i</sup> Argensola, History of the Spice Islands.

3d. No other country will correspond with his description of the Sinæ; "they were bounded on the north by part of Serica<sup>k</sup>, on the east and south by an unknown land, on the west by India, without the Ganges, according to the parallel already mentioned, and the great bay, and by the parts adjacent of the bay inhabited by wild beasts, and a part of the bay of Sinæ inhabited by Æthiopian fish-eaters: οἱ Σιναι περιρροζονται, απο μεν αρχων τῷ ἐκτε-  
 "βαιμένῳ μερὶ τῆς Σηρικῆς, ἀπὸ δὲ ἀνατολῶν καὶ μεσημέρας  
 "αἰγῶς γῆ. ἀπο δὲ δυσέως, τῇ ἐκ τῆς Γαγγῆς Ἰνδικῆς, καὶ  
 "τὴν διωρισμένην μετὰ τοῦ μεγάλου κόλπου γραμμῆν, καὶ  
 "αὐτῷ τῷ μεγάλῳ κόλπῳ, καὶ πρὸς ἐφεξῆς αὐτῷ κείμεναις,  
 "τῷ τε καλουμένῳ Θηρωδῇ, καὶ τῷ τῶν Σινῶν, οὐ περιροι-  
 "κουσιν ἰχθυοφάγοι Αἰθιοπες<sup>1</sup>."

We have already spoken of this unknown land to the east; but the land was unknown to the south likewise, not only according to this description of Ptolemy, but by a passage of Marcianus Heracleota, who lived after him, and had such other information as

<sup>k</sup> Ptolemy has placed this nation too far east, as is evident from a passage both in Dionysius and Rufus Festus Avienus. He has made an imaginary parallel intersect the 180th degree of longitude, and run north to the 63d degree of latitude, and bound both these nations to the east; and it would be difficult to conjecture the cause of this mistake, had these not been the most oriental nations he was acquainted with. This error appears to have bewildered all the moderns, who have attempted to ascertain the situation of this country. But as we hope to be able to prove that the present kingdom of Cambodia was the Sina of Ptolemy; the country of the Seres, by that rule, would be part of Thibet, and north of it to the 63d degree of latitude. But their true situation appears to have been in some part of the present Buckaria, to the east and north east of Samarcand.

<sup>1</sup> Lib. vii. c. 3.

discoveries produced during the interval from Ptolemy's death. He says, " we ought to conceive two  
 " unknown lands, one extending to the east, which  
 " the Sinæ possess, and the other towards the south,  
 " which stretches through the whole Indian ocean,  
 " so that both these unknown lands meeting, form  
 " as it were a certain angle in the bay of the Sinæ :  
 " δυο γὰρ αἰνῶτους ὑπονοεῖν χρὴ γὰρ, τὴν τε παρὰ τὴν ἀνα-  
 " τολὴν διηκουσαν, ἣ παροικεῖν εἰρηκαμένους Σινᾶς, καὶ τὴν  
 " παρὰ τὴν μεσημβριαν, ἣτις διηκεῖ παρὰ πᾶσαν τὴν Ἰνδι-  
 " κὴν θαλάσσαν ὥς τε συναπλουσας, ἐκείρας τὰς αἰνῶ-  
 " τους γὰρ, καθάπερ τινὰ γωνίαν ἀποδελεῖν περὶ τὸν τῶν  
 " Σινῶν κόλπον<sup>m</sup>." Now Cattigara stood upon a river which ran into this bay; and as they had never failed, so far as the mouth of the river Menam, the accuracy of this description is very apparent.

This country was bounded on the west by India, without the Ganges; or, as he has said elsewhere, by the 173d degree of longitude; extending north from the middle of the great bay to the country of the Seres, and the great bay, and parts adjacent of the bay of wild beasts, and part of the bay of the Sinæ, inhabited by Æthiopian fish-eaters<sup>n</sup>. These two bays were, by this description, evidently on the east side of the great bay, and between Cattigara and the bottom, or between the present Ponteamas and the mouth of the river Mecon.

For the same reasons therefore that the present Ponteamas seems to have been the ancient Cattiga-

<sup>m</sup> M. Heracleot. p. 29.

<sup>n</sup> Captain Hamilton says that the rivers of this country abound with many kinds of fish, which are a considerable object of trade among the inhabitants.



ra; the modern city of Cambodia will be the anti-ent metropolis Sinarum.

Ptolemy tell us, " that Cattigara lay south west  
 " of the metropolis: η δε απο της μετροπολεως των Σινων  
 " επι τον ορμον, τα Κατήλιαρα, προς δυσμους εσι, και με-  
 " σημεραν. ο" So does Ponteamals from the city of Cambodia.

He agrees with the author of the Periplus of the Red sea, " that this metropolis was a Mediterranean  
 " city, although he says it had no brazen walls, nor  
 " any thing worthy of notice: ουτε μεντοι χαλκα τειχη  
 " φασιν αυην εχειν, ουτε αλλο τι αξιολογον P."

The author of the Periplus says, " that beyond  
 " the Ganges, upon the eastern extremity of this  
 " country, under the very rising of the sun, there is  
 " an island in the ocean, having most excellent tor-  
 " toise shell, and all things that are to be found about  
 " the Red sea. And that after this country, imme-  
 " diately without it, in some place where the sea  
 " ends, is situated the greatest Mediterranean city,  
 " called Thina: κατ' αυην δε των ποταμων νησος εστιν ωκεαν-  
 " ειος, εχαλη των προς αναβολην μερων της οικουμενης, υπ'  
 " αυην ανεχοντα τον ηλιον, καλουμενη χρυση, χελωνην εχουσα  
 " παντων των καλα την Ερυθραν τοπων αρεσων. μελα δε ταυ-  
 " την την χωραν, υπ' αυην ηδη τον βορειαν εξωθεν εις Σινων τινα  
 " τοπον, αποληγουσης της θαλασσης, παρακειται εν αυη  
 " πολις μεσογειος μελιση λεγομενη Θινα Q."

This island in the ocean, which stood upon the ex-  
 tremity of this country, appears to be the present

\* Lib. i. c. 17.

P L. vii. c. 3.

Q Peripl. M. E. p. 36. Geog. Veter. Script. Oxon.

island of Sumatra, which fronts both the most southern and eastern parts of Malacca.

He has not in the least confounded this peninsula with the island, notwithstanding he calls them both by the name of Aurea; the first was upon the extremity of the east; the island also was upon the extremity of the east, but it was likewise under the very rising of the sun. Had he not been acquainted with this distinction, he would not have used the word *χωρον*, but *νησον* (so it would have been), after this island, but we see that he says it was, after this country, and immediately without it, in some place where the sea ended, where this city was situated.

This exactly agrees with the bay of Siam; it lies after Malacca, for we must pass that peninsula before we can arrive at it; it is also immediately without it, and towards the north in some place where the sea ends. The bottom of the bay of Siam is 13 degrees north of cape Romana, and there the sea may be said, without much impropriety, to end; more especially as in that place was situated this city, beyond which we have no accounts of any further investigations made in his days.

This very concise but accurate narrative, at the same time that it gives all the proof we can possibly expect that Sumatra was not at that time joined to the continent, so likewise does it demonstrate this metropolis to have been situated somewhere in this bay; and although we might conjecture, with as great an appearance of probability from these circumstances alone, that it was Siam, rather than Cambodia; yet from the collateral evidence already produced, that they sailed out of the bay before they arrived at its port, it is evident it lay south east of Siam,

and the testimony of Ptolemy and Marcianus Heracleota, who assert that the same south east course was held in order to reach Cattigara, being added, it necessarily follows, that that port was situated to the west of that cape which faces Puli Ubi<sup>r</sup>; but as there is no city west of that cape, on this particular part of the continent, except Cambodia, we may therefore reasonably suppose that city the ancient metropolis Sina<sup>s</sup>.

It may be observed that the ancient city of Thina and modern city of Cambodie not only agree in situation, but also in the nature of their produce. Formerly "they exported fine cotton and flowered

<sup>r</sup> See Plate IX.

<sup>s</sup> Vossius wondered how any one could doubt that the ancient Sina was the same with the modern Siam, without giving any reasons at all for his conjecture. "Quis dubitare possit, quin illa fit ea ipsa, quæ nunc Siam appellatur?" Voss. Obs. ad P. Melam, p. 560. Lugd. B. 1748. Whether or not Cambodia had the same name formerly with Siam, or was a province to it, is at this time impossible to determine; but it manifestly appears, that this metropolis was neither the present city of Siam, nor situated in any part of the country now known by that name.

<sup>t</sup> *Θανιον το σπρικον* seems to have been rather a vegetable, than vermicular production; for, although it may signify any species of web, it more properly denotes such as are made of cotton or lint; on that presumption, we have translated it flowered muslins; for, as these webs were made in the manner of those by a people who were called the Seres, it appears by the following lines that the figure of flowers were interwoven with theirs:

— *εθνεα βαρβαρα Σερων,*

*Ειμαλια τευχασιν πολυδαϊδαλα, τιμηνεντα,*

*Ειδομενα χροση λειμωνιδος ανθεσι ποιος.*

" *gentes barbara Serum*

" *Vestes faciunt, varii artificii, pretiosas*

" *Similes colore pratensis floribus herbæ.*"

Dionys. Periæg. ver. 752.

" muslins

“ muslins by land to the coast of Malabar and Guga-  
 “ rati : αφ’ ἧς τότε εἶλον, καὶ το σθονιον το σηρικον, εἰς του  
 “ ΒαρυΓαζαν δια Βακίρων περὶ φερῆαι, καὶ εἰς την λιμυ-  
 “ ρικην πολλω, δια του Γαγλου πόλαμου”. At present  
 “ they have cotton manufactures of various sorts,  
 “ white and painted calicoes, muslins, buckrams,  
 “ dimities, carpets, and silk tapestry, with other cu-  
 “ rious pieces, finer than any from the manufactories

— “ & plurima millia Serum  
 “ Illis nulla boum, pecoris, nec pascua curæ,  
 “ Vestibus utuntur texunt quas floribus ipsi.”

Perieg. Priscian.

These goods must have been more excellent than any which India, west of that city, at that time produced, or otherwise they would never have gone so far to seek them; from which it may be suspected that this invention of fabricating such beautiful muslins was exported with these merchandises, and is of much later date in Bengal than in this country to the east of it. Cambodia appears to have been called Rachmi by two Arabian travellers, who went to China in the ninth century; and even at that time the finest muslins in the world were manufactured by its inhabitants. In this same country, say the travellers, they make cotton garments in so extraordinary a manner that no where else are the like to be seen; these garments were for the most part round, and wove to that degree of fineness that they may be drawn through a ring of a middling size. M. Renaudot, Translat. 1733. Lond.

We have supposed that those kingdoms were the same, because Rachmi was contiguous to a kingdom seated upon a promontory, which appears to have been either the kingdom of Ava or Siam, to which the peninsula of Malacca was formerly a province; likewise there was but one kingdom between it and China, and north of it lay a country called Kascbin, which, by their description, appears to have been the kingdom of Laos. This will still be more probable when we consider that no country immediately to the east or west of it, viz. Pegu, Siam, or Cochin-China, are famed for these manufactories.

“ P. M. E. ubi supra.

“ in

“ in Holland ; besides, the inhabitants are the most  
 “ subtle merchants in the east, and the country itself  
 “ is superiour in fertility to most of those which  
 “ surround it \*.”

We are told “ that this city was difficult of access,  
 “ and that but a few people were acquainted with  
 “ the course in this voyage, and that these few seldom  
 “ failed so far : *εις δε την θιναι ταυτην ουκ εστιν ευχερως απ-*  
 “ *ελθειν. σπανιως γαρ απ’ αυτης τινες ου πολλοι ερχον-*  
 “ *ται.*,” it may be imagined that the great distance  
 of this city, and the irregularity of the winds in the  
 bay of Siam, which happens in all bays, was the oc-  
 casion of substituting these caravans in preference to  
 the voyage ; more especially as these two articles,  
 which are mentioned to have been exported, were  
 extremely light and portable, and could be easily trans-  
 ported by land to these countries already mentioned,  
 where they would be bought up, and dispersed over  
 Europe by the merchants of Ægypt.

But, notwithstanding we are ignorant of the time  
 when the route of these caravans was established, yet  
 we have no reason to presume, as an ingenious gen-  
 tleman has<sup>z</sup>, that this had taken place in the time of  
 any of the Ptolemies ; when no author, before the  
 emperor Trajan, has so much as mentioned this na-  
 tion. Indeed it is most probable this country was dis-  
 covered by such ships as that great prince sent to In-  
 dia, with a design of acquiring what information they  
 could receive ; in order, by their intelligence, to be as-

\* Argensol.

† P. M. Eryth.

z Schmidt, Opuscul. p. 184.

sisted in the designs he had upon that country, after his conquest of Arabia<sup>a</sup>.

There are remains of the pristine grandeur of Cambaia<sup>b</sup>; and its being famous in almost the same kind of produce is a strong indication of its obligations to the commerce of Cambodia. The marble ruins of an extensive city have lately been discovered<sup>c</sup> to the north west of Cambodia, and to all appearance in the very route of these caravans; but on this occasion, as on many others, we are but too sensible of the deficiency of intelligence, and of the great havock of time, which has involved the transactions of this period in almost impenetrable obscurity.

At the same time, that the antients extended their knowledge upon the continent, they must unavoidably have been acquainted with such of these islands as were most contiguous to it. Accordingly Ptolemy<sup>d</sup> has given us the names of several, in progression from the Ganges to Sumatra, which he has called, with two others he has joined to it, *Σαλαδελαι*; this supposition will appear more probable, as these islands are in the same longitude with Malacca, and directly south of that peninsula.

When they were upon that part of the coast which faces Malacca, they appear to have proceeded along the remaining part of the north east side of it, as far as Java, which he has named *Jabadiu*, *Ιαβადιου*<sup>e</sup>.

<sup>a</sup> Trajan died in the 118th of the Christian era.

<sup>b</sup> Hamilton.

<sup>c</sup> Argensol.

<sup>d</sup> Ptol. G. l. vii. c. 2.

<sup>e</sup> *Jabadiou* should perhaps be rendered *Javadiv*, as in the Malayan language *Dib* or *Dive* signifies an island, and *Giava* barley, which this island, according to Ptolemy, produced in great quantities. As *Giava* is derived from an old Persian word of the

And the inhabitants of this island were more civilized than some of the neighbouring ones, who were all cannibals, “ for it had a silver metropolis, and “ produced not only gold but plenty of barley: ὁ σῆ-  
 “ μαινει κελθος νησος. ευφωρωτάτη δε λεγέται ἡ νησος ειναι,  
 “ και ἐτι πλειστον χρυσον ποιεειν. εχειν δε μετροπολιν ονομα  
 “ αργυρου.”

Next to it were the three islands of Satyrs; “ they  
 “ were called so because the men who inhabited  
 “ them, were said to have such tails as the Satyrs  
 “ were usually painted with; ταυρίας<sup>f</sup> οἱ κατεχούτες,  
 “ ουρας εχειν λεγονται, ἵποιας διατρυφουσι τας των σατυρων.”  
 These were most probably the Celebes<sup>g</sup>, Borneo, &c.  
 “ for immediately after them were said to be other  
 “ ten islands (called Manillas), which produced such  
 “ large quantities of loadstone, as have been said to  
 “ attract ships ashore, which happened to be con-  
 “ structed with iron nails, for which reason the in-  
 “ habitants made use of wooden ones: φερωνται δε και  
 “ αλλα συνεχεις δεκα. εν αἷς φασι τα σιδηρους εχοντα ἡλους  
 “ πλοια κατεχεσθαι, καια της ἡραικλαιας λιθου περι αυτας  
 “ γεννωμενης. και δια τουτο επιρροις ναυπηγεσθαι, κατε-  
 “ χειν τε και αυτας ανθρωποφαγους, καλουμενους Μανιο-  
 “ λας<sup>h</sup>.”

same signification, it would appear that both the name of the island and that of the grain were exported from that country. Hyde's, Histor. Relig. Vet. Pers. &c.

<sup>f</sup> Ibidem.

<sup>g</sup> The Celebes is infested with numbers of mischievous and dangerous great monkeys, which keep in bodies too hard for any wild beasts to hurt them, and are only afraid of serpents, which pursue them to the very tops of the trees, and devour them. Bowen's Geogr. Dict. v. ii. p. 378.

<sup>h</sup> The Manillas are original names, and were called so by the inhabitants before the Spaniards took possession of them. Argensol, Histor.

Notwithstanding Ptolemy has mentioned the Philippines; yet we don't imagine that any of the persons from whom he acquired his information had ever been there: but that they had heard of these places at Java (to which they might easily have sailed), either from the Javanese themselves, or from the inhabitants of the circumjacent islands, who resorted to Java for the same advantages of commerce which they themselves came in pursuit of <sup>i</sup>.

However, although they must almost necessarily have been acquainted with Sumatra, yet it is evident they had never sailed quite round it; for, if they had, they would certainly not have mentioned Ceylon as the largest island in the ocean <sup>k</sup>.

From hence it would appear that they only knew part of Sumatra and Java; and either conjectured these were islands, or depended upon some informations they might probably have received from the inhabitants of these places, relative to this particular.

So that here we may venture to fix the limits of Ptolemy's knowledge; for, as these islands at that time were but a late discovery, they were very imperfectly known; and unfortunately the geographers, who lived after him, were all so prepossessed with his superior abilities, that they imagined his accuracy would bear no correction, and that he had exhausted the subject. For no other author mentions any discoveries to the east of these, taken notice of by him;

<sup>i</sup> The distance between Malacca and Java was less than between Malacca and Cattigara; besides, the south west monsoon was a fairer wind to the first than the last place, and of consequence this voyage must have been much shorter.

<sup>k</sup> Agathem. l. ii. c. c. 8.



and Marcianus Heracleota had such an opinion of his great merit, as to call him by the name “ of the “ most divine and most wise Ptolemy ; *ἐν τὰς γεωγραφίας του θειοτάτου και σοφωτάτου Πτολεμαίου.*”

By a retrospect on such authors as have been quoted, and some others who wrote nearly at the same time, according to the order in which they lived, this subject will still appear in a clearer light.

In the days of Strabo, who lived before the Christian æra, and is supposed<sup>1</sup> to have survived it 28 years, few people had sailed so far as the Ganges, “ *σπανιοι μὲν και πεπλευκασι μεχρι του Γαγγου* ”<sup>m</sup>;” and being intimately acquainted with Gallus, who was the third governor of Ægypt, he had undoubtedly the most favourable opportunities of the most authentic intelligence concerning naval affairs.

Pomponius Mela is supposed to have writ before Pliny<sup>n</sup>, in the reign of Claudius, and 30 years after Strabo. In that interval, there appears to have been made some farther discoveries upon the continent to the east of the Ganges; but so very imperfect, that they either imagined that country was an island, or had confounded their descriptions of it with these islands, which they would necessarily meet with in this voyage. For it is very certain, from Mela’s own words, that his knowledge of these places we are speaking of was extremely obscure, as all he has said

<sup>1</sup> Vossius.

<sup>m</sup> Strab. l. 15.

<sup>n</sup> Vossius, Bayle’s Hist. Dict.

• Strabo had probably the same information; but, as it was so extremely imperfect, did not think proper to mention it in his geography of this country.

of them is, “ ad Tabim insula est Chrysa, ad Gan-  
 “ gem Argyra, altera aurea soli, altera argentea ; at-  
 “ que ut maxime videtur, aut ex re nomen, aut ex  
 “ vocabulo ficta fabula est.”

The elder Pliny died in the 79th of the Christian  
 æra, and was a cotemporary of Mela ; and seems  
 to have referred to the above passage, in the follow-  
 ing words : “ Extra ostium Indi Chryse et Argyre,  
 “ fertiles metallis, ut credo ; nam quod aliqui tradi-  
 “ dere, aureum argenteumque iis solum esse, haud  
 “ facile crediderim p.”

Although the age in which Solinus lived is so un-  
 certain, yet it might be imagined that it was not  
 very long after Pliny ; having copied from the other  
 geographers which went before him, he has advan-  
 ced nothing upon this point that had not been alrea-  
 dy mentioned. His words are these : “ Extra Indi  
 “ ostium insulæ duæ, Chryse et Argyre, adeo sæcundæ  
 “ copia metallorum, ut plerique eas aurea sola pro-  
 “ diderint habere et argentea q.”

Josephus was 56 years of age, in the fourteenth year  
 of Domitian’s reign, or 93d of the Christian æra r ; and  
 appears to have had a little more knowledge of these  
 places than any we have yet mentioned ; for, speaking  
 of Saphira, from whence king Solomon had his gold,  
 he says, that “ it was a country of India, and not an  
 “ island ; and that it was now called by the name of  
 “ Aurea, νυν δὲ Χρυσήν γην καλουμένην, τῆς Ἰνδικῆς ἐστὶν  
 “ αὐτή.”

p J. Harduin, Plin. p. 322. vol. I. Paris, 1723.

q C. J. Solin. c. LII. Polyb. Histor. p. 700.

r Joseph. Oper. Ox. 1720.

Dionysius is supposed to have lived after Domitian, and before Severus<sup>1</sup>. He wrote a description of the world in Greek verse, which it may be supposed he had finished before the reign of Trajan, or at least that he had not heard of the increase of geographical knowledge which took place at that time, for he was as little acquainted with the country beyond the Ganges, as those who are supposed to have been his predecessors, and only mentions it as an island remarkable for the distinctness with which the sun-rising was observed,

*Χρυσειν τοι νησου αγει πορος, ενθα και αυτου*

*Ανολιη καθαροιο φαινεσθαι ηελιοιο.*

Dionys. Perieg. p. 111.

Ptolemy flourished under Adrian, and Antoninus; and made his last astronomical observation on a Wednesday, the 2d of February, in the year 141<sup>2</sup>. He has taken notice of many places not mentioned any where else, and is the first who has called Malacca a peninsula. Marinus indeed, whom he quotes as a late author, knew likewise that it was so; which still more confirms the supposition, that this was found out in Trajan's reign.

Ptolemy's works evidently shew, that his knowledge was superior to that of all the other antient geographers; and his living in Ægypt gave him ma-

<sup>1</sup> Dodwell, in Geog. vet.

<sup>2</sup> Universal Hist. Vol. XV. p. 206.

ny opportunities of a very early intelligence concerning any discoveries made by navigation, which might be a long time before they were communicated to the other learned men of that extensive empire. Accordingly we see, that the author of the *P. Maris Erythræi*, who is supposed to have been his cotemporary <sup>†</sup>, but lived a little later to the time of Marcus and Verus, was less acquainted with these late discoveries.

Agathemerus, who had read Ptolemy's works, lived in the reign of Severus and Galienus <sup>‡</sup>, in the beginning of the third century, and mentions the country of the *Sinæ* as the most oriental he was acquainted with.

Marcianus Heracleota is the last geographical author it will be necessary to mention. He is supposed to have lived some little time before the building of Constantinople, and even at that time this nation appears to have been the most oriental; for, although he copied from such authors as wrote in the interval between Ptolemy and him, yet all the improvement that was made during that time was only a men-suration of this particular coast, which Ptolemy himself tells us was not done in the days in which he lived <sup>\*</sup>.

From these circumstances it is apparent, that no mention was made of this country during the first century. Marinus, as we have seen, wrote before Ptolemy; Ptolemy was far advanced in years before

<sup>†</sup> Dodwell.

<sup>‡</sup> Dodwell.

<sup>\*</sup> Ptol. c. 14.

the middle of the 2d century; and farther, as it may be supposed that Trajan sent these ships to India at the time of his arrival in Arabia, which was in the 116th of the Christian era; this may very well agree, in point of Chronology, not only with these authors, but also with our former supposition, that this country was found out in his reign. But as he scarce survived the expedition two years, such persons as were employed in this voyage, finding on their return that he was dead, might be discouraged from pursuing any discoveries they had made: especially as the voyage was attended with so much hazard and difficulty, and as the views upon which they had undertaken it were in all probability frustrated by the accession of a new emperor.

Admitting therefore, that this was their first attempt, may not the extent of their discovery be looked upon as very considerable; and will it not in some measure account for their not having proceeded any farther than the east side of the bay of Siam?

Upon the whole, as nothing was exported from this kingdom of the Sinæ but what the city of Cambodia excelled in; and as the ancient and modern situations of these cities appear to be reciprocal; above all, as we have the testimony of the *Periplus Maris Erythræi*, that it lay somewhere in the bay of Siam, and the express declaration of two others, that it was situated on the east side of the bay; joined to the unanimous consent of all the geographers, that the country to the east and south was unknown, it may reasonably be inferred, that their *ultima* were upon

this coast; and the metropolis Sina or Thina the same as the modern city of Cambodia.

Greek-Street, Soho.

Feb. 1. 1767.

γ Θινα by the author of the Periplus Maris Erythræi, p. 36. Σινων μετροπολις and Θειναι by Ptolemy, lib. i. c. 17. Θειναι by Marcianus Heracleota, p. 14. Θιν, acervus, cumulus, collis, &c. it might receive this name because it stands upon a mount, according to Mandesloe, vol. I. p. 331. to secure it from inundations. None of all these ancient writers of geography have ever called this country Θινων but Agathameros, lib. ii. c. 7. and it would appear he did it by mistake; for its proper name was certainly Metropolis Σινων, and Θινα was only an accidental one, which it afterwards received to express more figuratively its situation.

Received February 12, 1767.

XVIII. *A Computation of the Distance of the Sun from the Earth.* By S. Horsley, L.L.B. Rector of Saint Mary, Newington, in Surry, F. R. S.

Read March 26, 1767. **I** Offer the following computation, rather as a verification than an amendment of Dr. Stewart's. The method, in which I have pursued, is different from what is used by that great and able geometrician, in his treatise on the distance of the sun, but founded entirely on the theorems established in that and the preceding tracts of the same author.

Let TA be a given line. Take Am, so that TA may be to Am, as the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take AG quintuple of Am. Take AP, such that twice Am may be to AP, as TG to TA. Now it is proved in the twenty-fifth proposition of Dr. Stewart's fourth tract, that the cube of TA is to the cube of TP, in the duplicate proportion of the periodic month to the anomalistic month. Therefore the proportion of TA<sup>3</sup> to TP<sup>3</sup>, and consequently that of TA to TP, is given; and by division, that of TA to AP is given. Therefore TA being given, AP is given. Now TG : TA = 2Am : AP. That is,

A a 2

TA

$TA = 5 \Delta m : TA = 2 \Delta m : AP$ . Therefore  $TA \propto 2 \Delta m$   
 $= TA - 5 \Delta m \times AP$ . Therefore  $2 \Delta m = \frac{TA \times AP - 5 \Delta m \times AP}{TA}$ .

That is  $2 \Delta m = \frac{5 \Delta m \times AP}{TA}$ , or  $\frac{2 TA + 5 AP \times m}{TA} = AP$ .

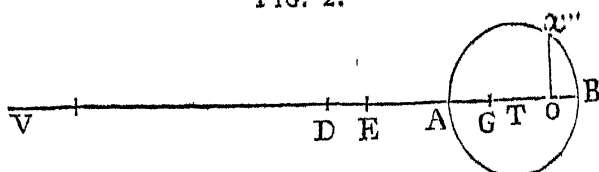
That is,

FIG. 1.



2. Let  $TA$  represent the moon's mean distance from the earth. Take  $TV$ , such that  $TA$  may be to  $TV$ , in the duplicate proportion of the periodic month to the fidereal year. Take  $TG$ , such that  $VT$  may

FIG. 2.



be to  $TG$  in the proportion of the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take  $TE$ , such that  $TE$  may be to  $TA$ , as  $TA$  to  $TG$ . Take  $EO$ , such that the rectangle  $EOA$  may be equal to  $3TE \times TA$ . Upon the centre  $T$ , with the interval  $TA$ , describe a circle. Draw  $Ox$  perpendicular to  $AB$ , meeting the circle in  $x$ . Take  $AD = AT$ . The proportion of  $TA$  to  $TV$  being given, and  $TA$  being given,  $TV$  is given. But the proportion of  $TV$  to  $TG$  is given. Therefore  $TG$  is given, and the proportion of  $TG$  to  $TA$  is given.  $TG : TA = TA : TE$ . Therefore the proportion of  $TA$  to  $TE$  is given. Therefore  $TE$  is given. Therefore  $3TE \times TA$  is given. Therefore  $EO \times OA$  is given. And  $EA (= TE - TA)$  is given. Therefore  $AO$  is given. But  $AB (= 2AT)$  is given. Therefore  $OB$  is given. Therefore  $AO \times OB$  is given.  $AO \times OB = Ox^2$  (by the circle). Therefore  $Ox^2$ , and consequently  $Ox$  is given. But  $DB (= 3AT)$  is given. Therefore the proportion of  $DB$  to  $Ox$  is given. And



And the proportion of DB to O $\propto$ , is that of the mean distance of the sun, to the mean distance of the moon.

This is in brief the method of my computation. The computation is as follows :

The periodic month is to the anomalistic month, as 57600 to 58091.

Therefore (in Fig. 1.) TA<sup>3</sup> : TP<sup>3</sup> = 57600<sup>3</sup> : 58091<sup>3</sup> = 3317760000 : 3374564281

$$\frac{3374564281}{3317760000} = 1,0171212748953155^{\circ}864197530^{\circ}864197530, \&c.$$

Hence, by extracting the cube root, I find TA : TP = 1 : 1,005674827053.  
Therefore put TA = 1. Then TP = 1,005674827053; and AP = 0,005674827053.

$$\text{Hence } \frac{TA \times AP}{2TA + 5AP} = 0,002797722 = Am.$$

(See Fig. 2). The square of the periodic month is to the square of the fidereal year, as 1 to 178,725.

Therefore TA : TV = 1 : 178,725.

But TV : TG = 1 : 0,002797722.

Therefore TA : TG = 1 : 178,725  $\times$  0,002797722 = 1 : 0,50002286445.

TA : TG = TE : TA. Therefore TE : TA = 1 : 0,50002286445.

Therefore put TE = 1.

Then TA = 0,50002286445

And EA = 0,49997713555

And 3 TE  $\times$  TA = 1,50006859335 = EOA.

$$\text{Hence AO} \left( = \sqrt[4]{TE \times TA + \frac{EA^4}{4}} - \frac{EA}{2} \right) = 1,00003658292.$$

But AB = 2TA = 1,00004572890

Therefore OB = 0,00000914598

Therefore AO  $\times$  OB = 0,0000091463145866546616

Therefore  $\sqrt{AO \times OB} = 0,0000091463145866546616$

But DB = 3TA = 1,500068593

Hence DB : O $\propto$  = 496,0073 : 1.

These computations have been made with no small rigor. I was sensible that, to obtain an accurate conclusion, it was necessary to determine AO with extreme precision; and for that purpose I submitted to the laborious task of computing the foregoing numbers to the 11th or 12th decimal place, by the common operations of arithmetic. In the result I differ from Dr. Stewart, by much less than  $\frac{1}{50000}$ th part of the whole distance, that is, by less than 5 semi-diameters of the earth; a very contemptible difference in so nice a calculation. That great mathematician indeed seems to have flattered himself, that he had determined the sun's distance within  $\frac{1}{40000}$  of the truth. I suspect that when he affirmed this, he did not consider that to attain so great an accuracy in the conclusion, the line  $Eg$  in his method (vide Stewart on the sun's distance, Fig. 10.), or AO in mine, should be determined strictly to the 11th or 12th decimal place. And after the utmost rigor of computation, I am afraid any pretensions to such extreme nicety in the result will be but ill-founded. For it is very likely that these computations represent the sun's distance less than it really is: because the whole progression of the moon's apogee (which is the basis of the calculation) is ascribed to the sun's disturbance of the moon's gravitation to the earth. Whereas part of it must be due to the disturbances of the planets. What part is due to them we cannot tell, and therefore cannot allow for it. But in giving the whole to the sun we certainly overrate his disturbing force, and by that means must obtain too small a distance.

It

It is most likely indeed, that the motion of the apogee produced by the disturbing forces of the planets bears but a very small, perhaps insensible, proportion to the whole. But those who are masters of Dr. Stewart's theorems will easily perceive that an insensible error in the proportion of the moon's gravity, and the sun's disturbance, may produce a very sensible error in the proportion of the mean distances. And therefore the real distance is probably greater by two or three semidiameters of the earth than these computations make it.

This, however, is much too nice a point for the approaching transit, or, perhaps, for any method of observation, to determine. The highest expectations astronomers will be answered, if they can come within 50 or 60 semidiameters of the earth.

It is to be hoped, that every civilized nation of the universe will give due attention to that interesting phenomenon, which we, the present possessors of these sublunary regions, shall behold no more; and that proper persons will be sent in due time, and duly equipped, to the most advantageous stations.

If the decisions of observation in so nice a point should be found to agree with the previous conclusions of theory, the disciples of Newton will have no small reason to exult in a new attestation of nature, to the truth of their great master's doctrine.

But it is much to be wished, that they, who shall be deputed to prosecute this curious search, in distant and sequestered parts, may divest themselves of all prejudice; that they may have nothing at heart, but, that which the world will expect from them, the

the advancement of real science; that they may be diligent in their observations, and faithful in their reports; and not sacrifice the repose of their own minds, or the interests of philosophy, to the credit of an admired hypothesis, the memory of a friend, or the jealousies of rival nations.

If the moon's mean distance from the earth be  $60 \frac{1}{2}$  semidiameters of the earth, the sun's mean distance is 30008,4416 semidiameters of the earth.

The sun's semidiameter is to the semidiameter of the earth, as 139,876 to 1. The globe of the sun is to the globe of the earth, as 2736718,8 to 1; and the sun's horizontal parallax is  $6'' 52'' 415$ .

February 8.

To satisfy myself more fully of the accuracy of my work, I this day re-computed the whole, from the determination of EA, in Dr. Stewart's approximating method. I found the proportion of DB to  $us$  (see Dr. Stewart on the distance of the sun, Fig. 10.), that of 496,00579 to 1; and the proportion of DB to  $tv$ , that of 496,00805 to 1. The mean of these two gives the proportion of DB to  $ox$ , nearly that of 496,0069 to 1. Which differs from the result of my former computation by less than  $\frac{1}{1240000}$  of the whole; and the method of the former computation is undoubtedly the most accurate.

*Supplement to the foregoing Paper.*

Read June 19, 1767. I N deducing the distance of the sun in femidiameters of the earth, and his horizontal parallax, from the proportion above concluded between the sun's mean distance and that of the moon; I have supposed the latter to be  $60\frac{1}{2}$  femidiameters of the earth, as it is reckoned by Sir Isaac Newton. According to the hypothesis which seems to be now generally received, that the density of the moon is very nearly equal to that of the earth, (the French reckon it rather less), the moon's mean distance should be little more than 60,23207, that is, not quite  $60\frac{1}{4}$  femidiameters of the earth. But from some computations that I have formed with great care; I have reason to think, that Sir Isaac Newton's determination is much nearer to the truth; that the density of the moon is actually greater than that of the earth, in the proportion of 6 to 5 nearly; and that the moon's mean distance amounts to 60,441 femidiameters of the earth; which differs from the distance assigned by Sir Isaac Newton, by less than  $\frac{1}{1000}$  of the whole.

S. Horfley.

Received February 12, 1767.

*XIX. Description of an improved Apparatus for performing Electrical Experiments, in which the Electrical Power is increased, the Operator intirely secured from receiving any accidental Shocks, and the whole rendered more convenient for Experiments than heretofore : By C. L'Epinafle, F.R.S.*

Read March 12, 1767. **T**HE first method of improvement consists in lining the inside of the glass cylinder or globe with the following composition.

Take 4 lb of Venice turpentine, 1 lb of resin, 1 lb of bee's wax; boil these over a gentle fire, stirring them now and then, for about four hours, at the end of which, stir in a quarter of a pound of vermilion : then, a little of the mixture being taken out and left to cool, will be hard and brittle; a token that it is fit for use. Having well heated your globe or cylinder, pour the melted mixture into it; turn the cylinder about so as to spread it evenly over the inside surface to the thickness of a fixpence, and let it cool very gradually.

The advantages that result from this are as follow.

1st, Upon repeated trials I have constantly found, that a cylinder thus lined acted with much greater force

force than it did before it was lined, every other circumstance alike. When first I made this observation, it induced me to try what effect the lining would have upon some cylinders, which I had found so bad that I had laid them aside as useless.

Upon being lined, they proved much better than any I ever had of the same size before.

2dly, Electrical machines, when laid by for any considerable time, are very apt to be out of order, and sometimes require much trouble before they can be brought to act: this inconvenience is in a great measure removed by thus lining the glass.

3dly, The cylinders thus lined are by far less liable to break by any alteration of weather, or in working the machine, which often was the case with mine before I lined them.

4thly, As a small cylinder thus prepared is equal in power to one much larger, that is not, and requires less friction, the apparatus in which it is mounted may be much contracted, and the whole, together with the person that turns the machine, may be easily supported upon one or two small stools with glass feet, when experiments require it. With a lined cylinder  $7\frac{1}{2}$  inches in diameter, and about 9 inches long, I have loaded three jars, that held four gallons each, to that degree as to burst one of them, which made an explosion near to that of a pocket pistol. The cylinder was mounted in a brass frame with a wheel and pinion; the wheel was turned with ease by a small brass winch, and the rotation of the winch to that of the cylinder was as one to three.

II. As raising the greatest quantity of electrical fire was the object of the first improvement, the next

thing was, to preserve it when raised, and use it without wasting any, so that it might have its full effect. I had observed, that whenever a single wire was made use of instead of a chain in discharging the jars, the effect was much stronger; and upon making further experiments, I found that when the discharging parts were not all in close contact, such as being screwed tight together, or ground into one another, the effect was considerably diminished. In constructing the discharging apparatus, I therefore contrived that all the parts should be in close contact, by screwing grinding, or soldering, them together; and thus the electrical fire exerts its whole force on the body upon which the experiment is made.

III. Lastly, it often happened in discharging the jars when loaded very high (as they must be to kill a large animal, or to force the fire through bodies that make a great resistance), that the persons operating, notwithstanding all their skill and care, received the whole or part of the shock. This has deterred many from repeating several useful experiments, and has intimidated others that attempted to repeat them so as to make them fail of their effect. To remove this inconvenience entirely, the discharging frame is contrived, which, at the same time that it prevents the wasting of the electrical fire, leaves no possibility of the operator's ever receiving any shock. This will plainly appear upon inspecting the figure annexed.

#### EXPLANATION of TAB. XI.

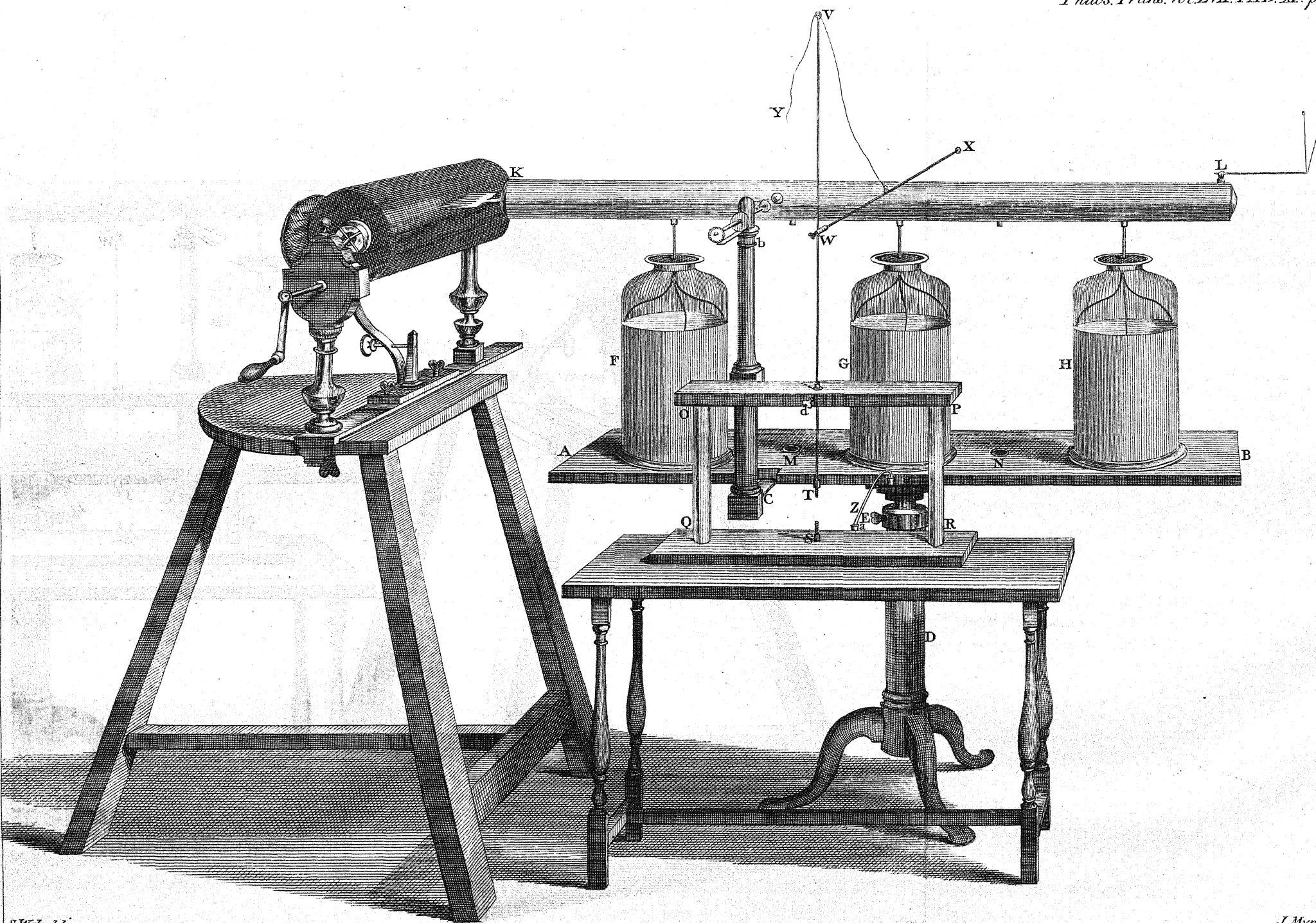
AB, a mahogany board  $4\frac{1}{2}$  feet long, which supports the jars F, G, H, and the conductor KL.

This









*S. Wale delin.*

*J. Mynde sculp.*



This board has three large holes cut through it, to fit the three jars: over these holes, on the under side of the board, are nailed three tin plates, which communicate to one another by a slip of tin, and upon which the bottom of the jars rest. Under the middle of this board is also fixed by a wooden screw a cylindrical piece of wood *c*, which moves up and down in the hollow of the clawed pillar *D*, and may be stopt at any height by means of the screw *E*; so that the whole apparatus can be raised to a height suitable to any electrical machine to which it is applied.

*F*, *G*, *H*, three glass jars, about ten inches diameter, and fourteen inches high, lined inside and out with tin foil to about two thirds of their height. A piece of wood is cemented at their top, through which there passes a thick brass wire, one end of which fits into a socket soldered to the conductor, and to the other end within the jar are fixed small wires, which spread and form a communication between the inside lining of the jars and the conductor.

*KL*, the conductor, a tin tube about five feet long and three inches in diameter, closed and rounded off at each end. At the end next the machine is fixt a piece of brass made of pointed wires in the shape of a comb, which collects the electrical fire from the machine; at the other end is fixt a slight piece of mahogany, at the end of which two small cork balls are suspended by fine threads, which balls by receding from each other, shew to what degree the power is raised; and when the jars are fully condensed will stand nearly upright as in the figure.

When experiments require to draw a spark from the conductor without a shock, which we may call single

single sparks, the jars are then removed, and the conductor is supported by two glass pillars, such as are used in the discharging apparatus; one end of which fits into holes made in the board at M and N, and the other end having a wire fixed to it, supports the conductor.

OPQR, the discharging frame placed on a table, and consisting of the following pieces.

QSR, a mahogany board twenty inches long and seven broad: about the middle of this board a narrow brass plate is let into the thickness of the wood, reaching from S to *a*. At the extremity *a*, which projects out of the board, there is a loop into which the end of the bent wire Z is fitted by being ground into it, the other extremity receives the end of the nut S.

S, a brass nut made with a double screw, the lower end being a male screw, to fasten it to the brass plate; and the upper end a female screw, to receive the ends of different wires and other pieces which fit into it for performing various experiments.

OQ, PR, two glass pillars about twelve inches long and an inch thick, made with shoulders at the end, which fit into the board QR, and the rail OP.

OP, a mahogany rail three inches broad and  $1\frac{1}{2}$  thick, nearly the same length with the board QR, which receives the upper ends of the glass pillars.

TV, a thick brass wire, which passes through the middle of the rail, and, by means of a screw at *d*, may be stopped at any height. At the upper end V of this wire is a loop, and at the lower end T a screw, to which pieces are fixed to answer those in the nut S.

W, a solid piece of brass, through which the upright wire TV passes, which may be shifted to any part

part of this wire suitable to the height of the conductor, and fixt there by a screw. A cross wire WX is fixt to this piece with a moveable joint towards W, so that it may be drawn up or let down by the silk string Y; one end of which is fixt to the cross wire, and the other end passes through the loop V.

X, a small brass knob at the end of the wire.

Z, a bent wire, which forms the communication between the discharging frame and the outside coating of the jars; one end of this wire is ground into a brass loop at *a*, and the other into a loop of the same kind at *e*, which is soldered to the tin plate that lies under the jars.

*bC*, an electrometer, which may be fixt on occasionally. When the discharging frame is used, the body upon which the experiment is made is placed between the nut S, and the end T of the upright wire, which is brought into contact with it; then the wire WX is drawn up to an erect position; and, after the jars are sufficiently loaded, it is let down upon the conductor by the silk string Y, and discharging the jars, the electrical spark in forming the circle passes through the body. Care must be taken to keep the glass pillars dry and clean, that none of the fire be wasted.

When an animal is to be killed, he must be fastened to a board, and his head placed between the two points S, and T. If the discharging frame be removed, the apparatus may be used in medical cases as usual; either with an electrometer or without.

XX. *Two Letters from the Hon. William Hamilton, His Majesty's Envoy Extraordinary at Naples, to the Earl of Morton, President of the Royal Society, containing an Account of the last Eruption of Mount Vesuvius.*

Naples, June 10, 1766.

My Lord,

Read March 5,  
and April 2,  
1767.

AS I have attended particularly to the various changes of Mount Vesuvius, from the 17th of November 1764, the day of my arrival at this capital, I flatter myself, that my observations will not be unacceptable to your lordship, especially as this Volcano has lately made a very considerable eruption. I shall confine myself merely to the many extraordinary appearances that have come under my own inspection, and leave their explanation to the more learned in natural philosophy.

During the first twelvemonth of my being here, I did not perceive any remarkable alteration in the mountain; but I observed the smoke from the mouth of the Volcano was much more considerable in bad weather than when it was fair; and I often heard (even at Naples, six miles from Vesuvius) in bad weather, the report of the inward explosions of the mountain. When I have been at the top of Mount Vesuvius in fair weather, I have sometimes found so little



little smoke that I have been able to see far down the mouth of the volcano, the sides of which were incrustated with salts and minerals of various colors, white, green, deep and pale yellow. The smoke that issued from the mouth of the volcano in bad weather was white, very moist, and not near so offensive as the sulphureous steams from various cracks on the sides of the mountain.

Towards the month of september last, I perceived the smoke to be more considerable, and to continue even in fair weather; and in october I perceived sometimes a puff of black smoke shoot up a considerable height in the midst of the white, which symptom of an approaching eruption grew more frequent daily; and soon after, these puffs of smoke appeared in the night tinged like clouds with the setting sun.

About the beginning of November, I went up the mountain; it was then covered with snow, and I perceived a little hillock of sulphur had been thrown up since my last visit there, within about forty yards of the mouth of the volcano; it was near six feet high, and a light blue flame issued constantly from its top. As I was examining this phenomenon, I heard a violent report, and saw a column of black smoke followed by a reddish flame, shoot up with violence from the mouth of the volcano, and presently fell a shower of stones, one of which falling near me, made me retire with some precipitation, and also rendered me more cautious of approaching too near, in my subsequent journeys to Vesuvius.

From november to the 28th of march, the date of the beginning of this eruption, the smoke encreased and was mixed with ashes, which fell, and did great

damage to the vineyards in the neighbourhood of the mountain. A few days before the eruption I saw (what Pliny the younger mentions having seen, before that eruption of Vesuvius which proved fatal to his uncle) the black smoke take the form of a pine-tree. The smoke that appeared black in the day-time for near two months, before the eruption had the appearance of flame in the night.

On good friday, the 28th of march, at 7 o'clock at night, the lava began to boil over the mouth of the volcano, at first in one stream; and soon after, dividing itself into two, it took its course towards Portici. It was preceded by a violent explosion, which caused a partial earthquake in the neighbourhood of the mountain, and a shower of red hot stones and cinders were thrown up to a considerable height. Immediately upon sight of the lava, I left Naples with a party of my countrymen, whom I found as impatient as myself to satisfy their curiosity in examining so curious an operation of nature. I passed the whole night upon the mountain; and observed that, though the red hot stones were thrown up in much greater number and to a more considerable height than before the appearance of the lava, yet the report was much less considerable than some days before the eruption. The lava ran near a mile in an hour's time, when the two branches joined in a hollow on the side of the mountain, without proceeding farther. I approached the mouth of the volcano, as near as I could with prudence; the lava had the appearance of a river of red hot and liquid metal, such as we see in the glass houses, on which were large floating cinders half lighted, and rolling one over another with

with great precipitation down the side of the mountain, forming a most beautiful and uncommon cascade; the color of the fire was much paler and more bright the first night than the subsequent nights, when it became of a deep red, probably owing to its having been more impregnated with sulphur at first than afterwards. In the day-time, unless you are quite close, the lava has no appearance of fire; but a thick white smoke marks its course.

The 29th the mountain was very quiet, and the lava did not continue. The 30th it began to flow again in the same direction, whilst the mouth of the volcano threw up every minute a girandole of red hot stones, to an immense height. The 31st I passed the night upon the mountain; the lava was not so considerable as the first night, but the red hot stones were perfectly transparent, some of which I dare say of a ton weight, mounted at least 200 feet perpendicular, and fell in, or near, the mouth of a little mountain, that was now formed by the quantity of ashes and stones, within the great mouth of the volcano, and which made the approach much safer than it had been some days before, when the mouth was near half a mile in circumference, and the stones took every direction. Mr. Hervey, brother to the earl of Bristol, was very much wounded in the arm some days before the eruption, having approached too near; and two English gentlemen with him were also hurt. It is impossible to describe the beautiful appearance of these girandoles of red hot stones, far surpassing the most astonishing artificial firework.

From the 31st of march to the 9th of april, the lava continued on the same side of the mountain in

two, three, and sometimes four branches, without descending much lower than the first night. I remarked a kind of intermission in the fever of the mountain, which seemed to return with violence every other night. On the 10th of april at night the lava disappeared on the side of the mountain towards Naples, and broke out with much more violence on the side next the *Torre dell' Annunciata*.

I passed the whole day and the night of the 12th upon the mountain, and followed the course of the lava to its very source; it burst out of the side of the mountain, within about half a mile of the mouth of the volcano, like a torrent, attended with violent explosions, which threw up inflamed matter to a considerable height, the adjacent ground quivering like the timbers of a water-mill; the heat of the lava was so great as not to suffer me to approach nearer than within ten feet of the stream, and of such a consistency (though it appeared liquid as water) as almost to resist the impression of a long stick, with which I made the experiment; and large stones thrown on it with all my force did not sink, but, making a slight impression, floated on the surface, and were carried out of sight in a short time; for, notwithstanding the consistency of the lava, it ran with amazing velocity; I am sure, the first mile with a rapidity equal to that of the river Severn, at the passage near Bristol. The stream at its source was about ten feet wide, but soon extended itself, and divided into three branches, so that these rivers of fire communicating their heat to the cinders of former lavas, between one branch and the other, had the appearance at night of a continued sheet of fire, four miles in length, and in some parts near

two

two in breadth. Your lordship may imagine the glorious appearance of this uncommon scene, such as passes all description.

The lava, after having run pure for about 100 yards, began to collect cinders, stones, &c. and a scum was formed on its surface, which in the day-time had the appearance of the river Thames, as I have seen it after a hard frost and great fall of snow, when beginning to thaw, carrying down vast masses of snow and ice. In two places the liquid lava totally disappeared, and ran in a subterraneous passage for some paces, then came out again pure, having left the scum behind. In this manner it advanced to the cultivated parts of the mountain; and I saw it the same night of the 12th, unmercifully destroy a poor man's vineyard and surround his cottage, notwithstanding the opposition of many images of St. Januarius, that were placed upon the cottage, and tied to almost every vine. The lava, at the farthest extremity from its source, did not appear liquid, but like a heap of red hot coals forming a wall, in some places ten or twelve feet high, which rolling from the top soon formed another wall, and so on, advancing slowly not more than about thirty feet in an hour.

The mouth of the volcano has not thrown up any large stones since the second eruption of lava, on the 10th of April, but has thrown up quantities of small ashes and pumice stones, that have greatly damaged the neighbouring vineyards. I have been several times at the mountain since the 12th; but as the eruption was in its greatest vigour at that time, I have ventured to dwell on, and I fear tire your lordship with, the observations of that day.

In my last visit to Mount Vesuvius the 3d of June, I still found that the lava continued, but the rivers were become rivulets and had lost much of their rapidity. The quantity of matter thrown out by this eruption, is greater than that of the last in the year 1760, but the damage to the cultivated lands is not so considerable, owing to its having spread itself much more, and its source being at least three miles higher up. This eruption seems now to have exhausted itself; and I expect in a few days to see Vesuvius restored to its former tranquillity.

Mount Etna in Sicily broke out the 27th of april, and made a lava in two branches, at least six miles in length, and a mile in breadth, and, according to the description given me by Mr. Wilbraham, who was there, after having seen with me part of the eruption of mount Vesuvius, resembles it in every respect, except that mount Etna, at the place from whence the lava flowed (which was twelve miles from the mouth of the vulcano), threw up a fountain of liquid inflamed matter to a considerable height; which, I am told, mount Vesuvius has done in former eruptions.

I beg pardon for having taken up so much of your time, and yet I flatter myself, that my description, which I assure your lordship is not exaggerated, will have afforded you some amusement. I have the honour to be,

My Lord,

Your Lordship's most obedient  
and most humble servant,

William Hamilton.

SINCE

Naples, February 3, 1767.

Read April 2, 1767. **S**INCE the account of the eruption of mount Vesuvius, which I had the honour of giving to your lordship, in my letter of the 10th of June last, I have only to add, that the lava continued till about the end of November, without doing any great damage; having taken its course over antient lavas. Since the cessation of this eruption, I have examined the crater, and the crack on the side of the mountain towards *Torre dell' Anunciata*; about a hundred yards from the crater from whence this lava issued: and I found therein some very curious salts, and sulphurs; a specimen of each sort I have put into bottles myself, even upon the mountain, that they might not lose any of their force, and have sent them in a box directed to your lordship, as you will see by the inclosed bill of lading. I am sure you will have a pleasure in seeing them analyzed. I have also packed in the same box some lava, and cinders, of this eruption; there is one piece in particular very curious, having the exact appearance of a cable petrified. I shall be very happy if these trifles should afford your lordship a moment's amusement.

It is very extraordinary, that I cannot find, that any chemist here has ever been at the trouble of analyzing the productions of Vesuvius.

The deep yellow, or orange-color salts, of which there are two bottles, I fetched out of the very crater of the mountain, in a crevice that was indeed very hot. It seems to me to be powerful, as it turns silver  
black

black in an instant, but has no effect upon gold. If your lordship pleases, I will send you by another opportunity specimens of the sulphurs and salts of the *Solfâ terra*, which seem to be very different from these.

Within these three days, the fire has appeared again on the top of Vesuvius, and earthquakes have been felt in the neighbourhood of the mountain. I was there on saturday with my nephew lord Greville; we heard most dreadful inward grumblings, rattling of stones, and hissing; and were obliged to leave the crater very soon, on account of the emission of stones. The black smoke arose as before the last eruption; and I saw every symptom of a new eruption, of which I shall not fail to give your lordship an exact account.



Received January 30, 1767.

XXI. *Extract of a Letter from John Howard, Esq; F. R. S. to William Watson, M. D. F. R. S. giving some Observations on the Heat of the Waters at Bath.*

Read April 2,  
1767.

WHEN I had the pleasure of seeing you in London, you thought some explanation of the paper, upon the heat of the Bath-waters, necessary. The observations, I assure you, were made with great care. The three first of them were made at the pump, where the waters are usually drunk. I went several times into the king's, and queen's, baths; and took them, where the springs rise in the king's bath, which is the warmest part, and the most distant from thence the coolest. The pump in that bath corresponded with the upper pump. This I mention; as in the other two baths, the thermometers did not rise, by one degree, so high as in the upper pumps; though I had the pumps worked a considerable time, to warm the pipes. The temperature of the other baths you see in my paper. The hot bath, not being so warm as the king's, at first, surprized me, until I considered their times of filling; and then the comparative slowness of the springs of the hot bath, to those of the king's, accounted for this difference.

The other observations are of various springs, in and about Bath; that near St. James's church is the

coldest. The springs, at the old and new well houses at Bristol, are very different; but, on repeated trials, I found them so.

*Observations on the Heat of the Bath Waters.*

King's bath pump	113°			
Hot bath pump	114			
Cross bath pump	108			
King's bath *	99	97	100	coolest part
	101	99	103	hottest part
Queen's bath *	97	95	98	coolest part
	98	96	99	warmest
The pump in the bath	113			
Cross bath	89			coolest part
	90			warmest part
Cross bath pump	107			
Hot bath	96			coolest part
	97			warmest part
The pump in the hot bath	113			
Pump in the Market-place, Bath				54
Springs on Claverton, and at late Mr. Allen's				47
Springs on Lansdown				45
St. James's spring water				43
Old well house, Bristol				67
New well, ditto				76

The temperature of the above springs taken in November, and December last 1765, by Farenheit's scale (Bird's Thermometer).

\* Taken at three different days.

John Howard.  
Bath,

XXII. *Observations on the Heat of the Bath and Bristol Water, by Mr. John Canton, A. M. F. R. S.*

Bath, Sept. 2, O. S. or the 13th N. S. 1752.

Read July 3,  
1767.

**A**FTER pumping about  $\frac{x}{4}$  of an hour, a Fahrenheit's thermometer, held in the stream from the common pump of the king's bath, was raised to  $112^{\circ}$ . The stream from the common pump of the hot bath raised it to  $114^{\circ}\frac{1}{4}$ . At the pump of the cross bath, it stood at  $110^{\circ}$ . At noon, the heat of the shaded air was  $66^{\circ}$ , and of common water exposed to it  $61^{\circ}$ . I found the bath water, and common water, brought to the same degree of heat, to cool equally fast. The next day, Sept. 14, I was at the hot-well near Bristol, the water of which raised the thermometer to  $76^{\circ}$ . In common water exposed to the shaded air it stood at  $62^{\circ}$ .

J. Canton.

Received April 3, 1767.

XXIII. *A Letter to Dr. William Watfon;  
F. R. S. from the Hon. Daines Barrington,  
F. R. S. on some particular Fish found in  
Wales.*

Dear Sir,

Read April 9, 1767. **K**NOWING your attention to every thing which relates to any branch of natural history, I shall not make apologies for sending you an account of the following particulars with regard to perch in a pool of Merionethshire; and trout, which are found in a river of Cardiganshire.

The pool is situated in the parish of Trawsfynydd, and is called Llyn Raithlyn; as it does not lie near any road, a common traveller cannot hear any thing about it, but by very extraordinary accident.

Having been informed by Mr. Garnons of Riwgoch, who lives near this pool, that perch were frequently caught there, which were crooked near the tail, I have, through him, procured fish of this sort at three different times; as I intended to preserve them in spirits, I have always desired that they should be of a small size.

These fish were all of them most apparently crooked in that part; which appears still stronger (as I am informed) in those of a larger size, and some of them have been taken of nearly two pounds.

I have never examined the back bone of these perch, but I have now by me that of one of the trout (which I mean likewise to give you an account of): this bone, any one may immediately see, differs most apparently from that of a common trout, or any other fish, by its being crooked near the tail. I have therefore no doubt but that the back bone of these perch will turn out to be equally crooked.

These fish are not only crooked near the tail, and for about one third of the whole length of their body; there is likewise a very remarkable protuberance on each side, which I have opened with a knife, but did not observe it to differ materially from other parts of the flesh. I have likewise eat these perch, and should not, by the taste, have distinguished them from the common ones of the same kind.

I happened likewise (after these inquiries with regard to the perch of Llyn Raithlyn) to hear of trout, which were crooked in the same part, said to be peculiar to the river Eynion in Cardiganshire, which is a small brook, that empties itself into the Dovey, near Egglwys Vach, and is on the road from Machentleth in Montgomeryshire, to Talypont in Cardiganshire\*.

\* In Dalekarlia, a province of Sweden, near Fahlun, are two small lakes, famous for the singular shape of the perch, where-with they abound. These perch grow to the common size, and are of a good taste, but they have all a hump on their back. This peculiarity is taken notice of in Linnæi Fauna Suecica, † p. 118. The country people in the neighbourhood imagine that it may be occasioned by the quality of the water in those lakes, which

† In stagnis Fahlunæ hujus piscis (Perce) varietus est, quæ si inâ recurvâ, & corpore omnino gibbo, frequens reperitur.

I have procured at two several times specimens of these trout likewise, and have one now by me in spirits. They are crooked in the same manner near the tail; but, as the make of a trout is more taper than that of a perch, the curve does not appear so strongly: no one, however, who looks at them with any degree of attention, can have the least doubt of their differing most materially, from other fish of the same kind.

These trout are caught, only in a small basin of perhaps eight or nine feet deep, which the river Eynion forms after a fall from the rocks. I have been informed, that, in a calm day, you may often see them in this basin; it hath so happened that a good deal of wind hath ruffled the surface, when I have examined it.

I have only to add to these particulars, that, by very accurate accounts from those, who have caught both the perch and trout, it is not above half of these fish which are thus crooked; and that the others do not in any respect differ from the common ones of these two sorts.

As I have often observed that the existence of such fish was doubted by the Welsh themselves, till I had procured these specimens, it hath occasioned my

might probably be impregnated with some mineral salt, especially as they are situated near the largest copper mine in Europe.

Dan. Solander.

There is no copper mine near Llyn Raithlyn, or the river Eynion.

Daines Barrington.

inquiring

inquiring with regard to monocular fish, which are said by Giraldus Cambrensis, to be found in the lakes of Snowden, and from whom I shall transcribe the whole passage :

“ In summis autem montium istorum verticibus,  
 “ duo lacus reperiuntur, suâ non indigni admirati-  
 “ one : alter enim insulam habet erraticam, ad oppo-  
 “ sitas plerumque partes errabundam ; alter vero miro  
 “ & inaudito miraculo pollet, quia cum trium gene-  
 “ rum piscibus abundet, anguillis, truttis, & perchiis,  
 “ omnes in eo pisces monoculi reperiuntur, oculum  
 “ dextrum habentes, & sinistro carentes : si rei tam  
 “ novæ, tamque stupendæ rationem scrupulosus lector  
 “ efflagitet, assignare non præsumo.” Giraldus  
 Cambrensis, lib. ii. cap. 10.

This writer was Archdeacon of Brecknock, and attended Baldwin Archbishop of Canterbury, in a progress which he made in the year 1188 through South and North Wales, to recommend a collection for a crusade which was then in agitation.

We have no account of any part of England nearly so ancient, Leland's *Itinerary* having been undertaken only in the reign of Henry the VIIIth : it is not however merely the antiquity of the work, which should recommend Giraldus's observations to the perusal of every English reader ; they are at the same time very entertaining, nor is the latinity contemptible.

Giraldus hath in common with other ancient travellers been considered as taking the liberties, with which they have been so frequently charged. For my

own part the last undoubted discovery of the Patagonian giants, mentioned, but not credited in any previous account, will teach me not to disbelieve entirely what is not a contradiction, on the very state of it. I therefore do not absolutely disbelieve Mr. Grose's late description of pigmies which are found in a forest of the Carnatic, though I admit such facts require the strongest testimony before one should give a complete assent. In short, I am neither for implicit belief or disbelief of such extraordinary facts; and it is remarkable that Aristotle, in his account of a nation of pigmies, says, "this is not a fable, but a truth."

Εστι δὲ ὁ τόπος αὗτος, περὶ ὃν οἱ Πυγμαῖοι κατοικοῦσιν· οὐ γὰρ ἐστὶ τοῦτο μῦθος, ἀλλὰ ἐστὶ καὶ τὴν ἀληθειαν, γένος μικρὸν μὲν (ὥσπερ λεγέσθαι), καὶ αὐτοὶ, καὶ οἱ ἵπποι. Arist. de Nat. anim. l. viii. cap. 12.

But to return to the extract from Giraldus which hath been looked upon as one of the most glaring falsities, in this traveller.

It will appear to any one who reads the whole of his *Iter*, and is at all acquainted with the geography of the country, that Giraldus (who was a native of Pembrokeshire) never was himself in these mountains of Snowden; he had therefore only picked up this account, from some of the inhabitants of the towns, through which the Archbishop passed, who themselves probably received it from mountaineers.

There are few inhabitants of the principality, who have ever been in this tract of mountains; and I, who have been in most parts of them, have always been informed, at my setting out, that the roads were nearly unpassable.

Upon



Upon these occasions, I have frequently inquired whether there was any such notion or tradition amongst the mountaineers, with regard to monocular fish, and have found, that it is supposed there are such in a pool called Llyn y Cwn, which indeed I have never seen; but, by the best accounts I can procure, it is high up the Glyder mountain, which forms the opposite side of the vale of Lanberris to Wyddva, or the highest part of Snowden.

I have, by means of an interpreter, examined some of these mountaineers very particularly with regard to this point. One of them told me that, though he had often heard of these monocular fish, yet he had seen two or three taken by an angler in Llyn y Cwn, which did not seem to differ from common trout; and indeed the fish of that lake are seldom caught, as they are of very difficult access, and have no extraordinary character either for goodness or size.

Mr. Hughes (of Penrhyn in Carnarvonshire) having known that I had a curiosity with regard to these extraordinary fish, sent me over last summer a mountaineer, whose account of them I took down by means of an interpreter, and send you herewith.

“ Thomas William, of the parish of Lanlechyd,  
 “ taylor, aged 51, remembers one Daniel Pritchard of  
 “ Comb Cloran, yeoman, who was near 100 years  
 “ of age, and died in 1764. This Daniel Pritchard  
 “ told him (about nineteen years since), that he  
 “ caught a trout at Llyn y Cwn with one eye only in  
 “ the forehead, and that the head was thicker than  
 “ the heads of trout commonly are. He added also,  
 “ that William Robert, a fuller, had heard the same  
 “ story from Pritchard: Thomas William himself,  
 “ however

“ however informed me, that he had been fishing in  
 “ this pool, but never caught a trout of this extraordi-  
 “ nary kind. He had likewise fished in Llyn March-  
 “ leyn, and Llyn Bochlwyd, and though he caught  
 “ some fish with crooked backs, never took any that  
 “ had but one eye. He concluded his account by  
 “ saying, that there is a brother-in-law of Pritchard  
 “ now alive, who will confirm Pritchard’s testimony ;  
 “ and that it is generally believed in Lanleched  
 “ parish, there are fish which have but one eye, in  
 “ some of the pools of the neighbouring mountains.”

If by this I have not established the existence of  
 such monocular fish in the Carnarvonshire lakes, I  
 hope you will at least think that Giraldus’s account is  
 not to be so immediately rejected as impossible, espe-  
 cially when I send you an extract from the History  
 of the French Academy of Sciences, which contains  
 the following proof of nearly as extraordinary fish in a  
 lake of France.

“ Monsieur le Marquis de Montalembert a fait à  
 “ l’Académie l’observation suivante. Dans la fontaine  
 “ du Gabard en Angoumois, on pêche souvent des  
 “ brochets aveugles, & jamais aucun qui ne soit  
 “ borgne. Ceux qui ne sont que borgnes, le sont tous  
 “ de l’œil droit, & dans ceux qui sont aveugles on voit  
 “ aisément que l’œil droit a été attaqué le premier,  
 “ & est beaucoup plus endommagé que l’autre.  
 “ Cette fontaine est une espèce de gouffre, dont on ne  
 “ peut trouver le fond ; plusieurs petites îles de  
 “ roseaux, qui flottent à sa surface, empêchent qu’on  
 “ ne puisse se servir de filets, pour y pêcher, ce qui  
 “ rend cette pêche très longue, & très difficile.  
 “ M. de Montalembert fut assez heureux pour at-  
 “ traper,

“ traper un jeune brochet, qui effectivement se trouva  
 “ borgne du coté droit. Ce qu’il y a de singulier  
 “ c’est que cette fontaine se décharge par un assez  
 “ gros ruisseau dans la Liffonne, & que malgré cette  
 “ communication, qui est très facile, les gens du pays  
 “ affurent qu’on ne prend jamais dans cette riviere de  
 “ brochets borgnes, ou aveugles, & qu’on n’en prend  
 “ aucuns dans la fontaine qui ne le soient.”

I refer you for this extract to page 27 and 28, of the History of the Académie of Sciences for the year 1748, being the quarto edition, which you was so good as to lend me on this occasion \*.

The latter part of this extract, which asserts that these blind pike are only to be found in the pool of Gabard, and not in the small river by which it communicates with the Liffonne, suggests to me that it is generally supposed (and even by Lhwyd in his additions to Camden’s Britannia) salmon are never caught in the lake of Bala in Merionethshire, though they are frequently taken in the river Dee, just below where it issues from that lake; whilst the contrary is observed with regard to the fish called a Gwyniad, which is at the same time conceived to be peculiar to this lake.

I happened myself once to see a salmon of about fifteen pound, caught in the lake, at least 200 yards

\* I am likewise referred by an ingenious friend to a passage in Fr. Ern. Bruckmanni *Epistolæ Itinerariæ* xxxvi. Wolfenb. 1734, p. 10, which mentions a river in Germany, having all the trout blind: “Truttæ omnes (teste P. Stephan. Amiodt, de Germaniâ “ in naturæ operibus admirandâ, p. 66.) in flumine *Fischau* prope “ *Mandorf* visu destitutæ dicuntur. Vide plura apud Kinkelbeck, “ p. 809, E. Brown in *Itin.* p. 196. and Math. Puel in *Itin.* “ *Thalassico*, p. 33.”

above the bridge through which the river Dee issues ; and though I never saw the Gwyniad taken in the Dee, yet I was most authentically informed by a gentleman who was present, that several of them were caught within these three years as low down as Landrillo, which is near six measured miles from the lake of Bala.

With regard to the Gwyniad's being peculiar to this lake, which is so generally believed, I can myself most flatly contradict this notion, as the first parcel of fish, which I happened to see in the market at Perith, in Cumberland, were of this sort, and were brought from the Ulles Water, which is a large lake not above four or five miles distant from that town.

I have been likewise informed, by some people, who lived on the banks of Loch Lomond in Scotland; that fish answering to my description of a Gwyniad were often caught in that lake. I did not happen however, myself, to see any of this sort.

There is indeed one very striking mark in this fish, which cannot but be attended to by those even who are not naturalists : they have ventral fins of a very deep blue ; and the belly, at most seasons, is marked with small blue specks, which I do not recollect to have observed in any other fish of this island.

I have thus endeavoured to contribute my poor mite with regard to the natural history of Wales, which I have been enabled to state with some precision, from having had occasion to go twice a year into the principality, and to stay six or seven days in a particular place.

When I have mentioned to some friends, who are versed in botany, and the study of natural history, the necessity

necessity of a more accurate and complete history of this kind ; I have been frequently answered, that they despaired of making any material additions to the discoveries of Ray and other naturalists, who have been in many parts of this country.

It is very true that Ray and some of his contemporaries were in many of the Welsh counties; it appears, however, by his Journal lately published, that he stayed but a very short time at any place, and only went to the top of some of the high mountains.

Now, Sir, I need not say, to a person of your consummate knowledge in botany, that to discover all the plants of a country, one should at least, once a fortnight, search every part, from the time vegetation commences, to that period of the autumn when it ceases. He who searches for pilewort in july, without knowing that it is a plant, which appears early in the spring, and that both leaves and flowers immediately afterwards decay, will search in vain : the botanist must look for it in april, or not at all.

Dillenius is the latest botanist of reputation, who hath visited these mountains; as I remember to have seen him a year or two before his death, I can venture to say that those who may afterwards make the same search, need not absolutely despair of new and material discoveries, notwithstanding his known diligence and accuracy.

He was of a very improper make for clambering up a rock, or mountain; and indeed it is a misfortune to the naturalist, that when from experience his knowledge becomes considerable, his strength and activity begin to fail.

When,

When a stranger, from botanical or other curiosity goes to the top of a Welsh mountain, he is obliged to trust implicitly to his guide, who contrives to carry him where he can ride on horseback. The stranger therefore proceeds in the very track, where a sheep or cow can graze, which consequently crop the flowering stem and leaves of the plants.

It is in the fissures of rocks, and the central parts of bogs, that the search for uncommon plants must be made; he who looks elsewhere for them, may travel a great deal of ground, but will never make any material discoveries.

I will not detain you longer with observations of this kind, as they must have been to you long obvious and familiar. I have, however, been insensibly betrayed into this length, by thinking, that every attempt to render the natural History of Great Britain more complete, and accurate, may not be entirely without its use.

I am,

Dear Sir,

Your most faithful

Humble servant,

Daines Barrington.

**XXIV.** *An Observation of an Eclipse of the Sun at the Island of New-found-land, August 5, 1766, by Mr. James Cook, with the Longitude of the Place of Observation deduced from it: Communicated by J. Bevis, M. D. F. R. S.*

Read April 30,  
1767.

**M**R. Cook, a good mathematician, and very expert in his business, having been appointed by the Lords Commissioners of the Admiralty, to survey the sea coasts of New-found-land, Labradore, &c. took with him a very good apparatus of instruments, and among them a brass telescopic quadrant made by Mr. John Bird.

Being, August 5, 1766, at one of the Burgeo Islands near Cape Ray, latitude  $47^{\circ} 36' 19''$ , the south-west extremity of New-found-land, and having carefully rectified his quadrant, he waited for the eclipse of the sun; just a minute after the beginning of which, he observed the zenith distance of the sun's upper limb  $31^{\circ} 57' 00''$ ; and, allowing for refraction, and his semidiameter, the true zenith distance of the sun's centre  $32^{\circ} 13' 30''$ , from whence he concluded the eclipse to have begun at  $0^h 4' 48''$  apparent time, and by a like process to have ended at  $3^h 45' 26''$  apparent time.

N. B. There

N.B. There were three several observers, with good telescopes, who all agreed as to the moments of beginning and ending.

Mr. Cook having communicated his observation to me, I shewed it to Mr. George Witchell, who told me he had a very exact observation of the same eclipse, taken at Oxford by the Rev. Mr. Hornsby; and he would compute, from the comparison, the difference of longitude of the places of observation, making due allowance for the effect of parallax, and the earth's prolate spheroidal figure; and he has since given me the following result:

5 <sup>h</sup> 23' 59"	beginn. at Oxford.	7 <sup>h</sup> 7' 5"	end at Oxford.
0 46 48	beginn. at Borgeo Isles.	3 39 14	end at Borgeo Isles.
<hr/>			
4 37 11		3 27 51	
— 51 59	effect of parallax, &c.	+ 17 35	effect of parallax, &c.
<hr/>			
3 45 22	diff. of meridians.	3 45 26	diff. of meridians.

J. Bevis.



XXV. *Letter to Mr. Dacosta, Librarian, &c. to the Royal Society, from Mr. William Martin; containing an Extract of a Letter from his Son at Bengal, on the Heat of the Climate.*

S I R,

Read April 30, 1767. **T**HE candid reception I met with, when I took the liberty of addressing you in November 1764 by my friend John Ellis, Esq; a case somewhat uncommon, emboldens me, at this time, to communicate part of a letter I have lately received from my son, Fleming Martin, Esq; chief engineer at Bengall, dated 1st october 1765; giving an account of the incredible heat attending that climate, with some other observations, &c. If you think it worthy of being communicated to the Royal Society, it will afford me the greatest pleasure; but this I must beg leave to submit to your judgment, and am, with great respect,

Sir, your most obliged

humble servant,

Shadwell, 30 April,  
1766.

William Martin.

*Copy of part of a Letter, from Fleming Martin, Esq;  
chief Engineer at Bengall, dated 1<sup>st</sup> October, 1765.*

**I**N regard to the intense and uncommon heat in this climate; it has been for some time past almost insufferable.

The thermometer was seldom under 98, and the quicksilver rose at certain times of the day to 104 degrees, by the best adjusted instrument; nay, I have been assured by some gentlemen, that, in the camp 500 miles distant, the thermometer often stood at 120; but such a difference, I imagine, was occasioned by the badness of the instrument.

However it is certain, that nothing could exceed the intense heat we felt day and night, during the month of june. May and july were little inferior at times, but afforded some intermission; otherwise a very great mortality must have attended this settlement, though we were not without instances of fatal effects in the month of june, when some few individuals in sound health were suddenly seized, and died in the space of four hours after; but, considering the malignity of the climate, we have not lost many, and I believe the generality of people are not so intemperate as some years past they used to be; though, from what I have seen, the best constitutions in the most moderate persons are a poor match against a fever or other disorders in this country.

I have been as free from sickness, as any other person in the settlement; but I cannot say that I have enjoyed myself in that degree as to be an exception; for no man here is without complaints, and life and  
death

death are so suddenly exchanged, that medicines have not time very frequently to operate before the latter prevails. This is generally the case in malignant fevers, which are here termed *pucker fevers*, meaning (in the natives language) strong fevers.

The rains have set in since the 4th of june. We call this the unhealthy season, on account of the salt petre impregnated in the earth, which is exhaled by the sun, when the rain admits of intervals. Great sickness is caused thereby, especially when the rains subside; which generally happens about the middle of october. The air becomes afterwards rather more temperate, and, till april, permits of exercise, to recover the human frame that is relaxed and worn out by the preceding season; for in the hot periods every relief is denied, except rising in the morning, and being on horse back by day break, in order to enjoy an hour, or little more, before the sun is elevated: it becomes too powerfull by six o'clock to withstand its influence; nor can the same be attempted that day again till the sun retires, so that the rest of the twenty-four hours is passed under the most severe trials of heat. In such season it is impossible to sleep under the suffocating heat that renders respiration extremely difficult; hence people get out into the virando's and elsewhere for breath, where the dews prove cooling, but generally mortal to such as venture to sleep in that air. In short, this climate soon exhausts a person's health and strength, though ever so firm in constitution, as is visible in every countenance, after being here twelve months. I have been lately informed by an officer of distinction, who was formerly engineer at this place, that he being sent out to survey

a salt lake in the month of September, he found the sulphureous vapours so stagnated and gross, that he was obliged to get up into the tallest trees he could find, to enjoy the benefit of respiration every now and then; he added that he constantly had recourse to smoking tobacco, (except during the hours of sleep), to which and to swallowing large quantities of raw brandy (though naturally averse to strong liquors), he attributed his safety. However, on his return, he was seized with an inveterate fever of the putrid kind, which he miraculously survived, though others, who attended him on the survey, and had lived many years in the climate, were carried off, at the same time, by the like fever.

Received February 5th, 1767.

XXVI. *Experiments on the Peruvian Bark,*  
by Thomas Percival, M. D. F. R. S.

EXPERIMENT I.

Read March  
7, 1767.

**A**N ounce of Bark coarsely powdered was divided into two equal parts, one of which was infused forty-eight hours in cold spring water; the other was boiled over a slow fire forty minutes, until about a third part of the water was evaporated. The infusion and decoction were each filtered through linnen rags doubled and of equal fineness.

Four grains of *Sal Martis* were dissolved in an ounce of spring water, and one dram of this solution was added to equal quantities, viz. half an ounce; of the turbid decoction and infusion. Each assumed a deep purple colour, scarcely perceptibly different in degree; though I thought the infusion, after standing a while, acquired rather a more dusky purple than the decoction.

The infusion had a deeper tinge, and more of the taste and smell of the Bark in substance than the decoction. Its taste indeed exactly resembled the Bark, after it had been broke down and chewed for some time in the mouth.

EXPE--

## EXPERIMENT II.

Equal quantities of each residuum were boiled in three ounces of spring water over a slow fire, for the space of twenty minutes. The decoctions were equally turbid, exactly similar in taste; and on the admixture of the chalybeate solution in the proportion of one dram to half an ounce, they assumed precisely the same colour, namely a dusky brown, like chocolate, but inclining somewhat to purple.

## EXPERIMENT III.

Five ounces of each residuum were infused, for the space of forty hours, in an ounce and a half of Jamaica rum, which was sufficiently pure, and unimpregnated with any astringent matter from the cask. The tinctures were exactly alike in taste and colour; and on the addition of one dram of the chalybeate solution, they were instantly changed from a deep red, to a dark and dirty brown; which was precisely the same in both tinctures.

## EXPERIMENT IV.

To half an ounce of powdered Bark, was added an ounce of cold spring water. The mixture was well triturated in a marble mortar, after which it was suffered to remain at rest until the gross powder subsided. The clear liquor was then carefully poured off, and fresh water to the quantity of half an ounce was added; the trituration was renewed, and afterwards part of the menstruum poured off again as before. This method was pursued for the space of

of thirty-four hours, in which time six ounces of water were combined with the Bark. The mixture was then infused fourteen hours without heat; and strained off. This infusion was found to have the smell and taste of the Bark, in a considerable greater degree than either the decoction or infusion without trituration of Experiment I. and it assumed a much blacker colour, on the admixture of one dram of the chalybeate solution, than either of the two former preparations.

#### EXPERIMENT V.

It was attempted to determine the comparative strength, or rather astringency, of five preparations of the Bark, viz. the extract, decoction, cold infusion, tincture and triturated infusion. Ten grains of the extracts carefully made, and as free from empyreuma, as this officinal preparation is generally found to be, were mixed with an ounce of hot water. But so imperfect was the solution, or, to speak more properly, the suspension of the Bark, that in a few minutes a large powder was deposited at the bottom of the glass. This however was shaken up, and one dram of the chalybeate solution was added to the mixture. The same quantity was added to half an ounce of the decoction, infusion, tincture, (Pharm. Lond.) and triturated infusion; the last assumed by far the deepest black; the extract approached nearest to it, and the tincture appeared to be the least tinged. The decoction and infusion were precisely alike in colour.

## EXPERIMENT VI.

The residuum of the triturated infusion, Experiment IV. was boiled over a slow fire in three ounces of water, for the space of twenty minutes. The decoction when cold was strained off, it was of a paler colour than the decoctions mentioned Experiment II. although there was a portion of powdered Bark, suspended in it, which, by the trituration, had been rendered fine enough to pass through the filter. This powder, on standing, subsided to the bottom of the vessel, and left the decoction much more limpid than it was before.

To equal quantities of this, and of the two decoctions mentioned above, one dram of the chalybeate solution was added; the black tinge was manifestly weakest in this decoction, though the difference was not so great as might have been expected, from the diversity in their sensible qualities of taste and smell; owing perhaps to the fine powder of the Bark, which floated in it, and retained some degree of its original astringency.

## EXPERIMENT VII.

Equal quantities of the simple and triturated infusion were boiled for the space of seven minutes over a quick fire; both lost their transparency when cool; but the latter assumed a much more turbid appearance than the former, exceeding even that of the decoction from fresh Bark, Experiment I. and, after standing twenty-four hours, it deposited a very copious sediment.



## EXPERIMENT VIII.

Half an ounce of powdered Bark was infused forty-eight hours in five ounces of spring water, and one ounce of white wine vinegar. The mixture was placed near a warm fire, and frequently shaken up. It was then filtered through a linen rag doubled. The taste of the vinegar was in a good measure covered, though the smell was not; but the menstruum was not so fully impregnated with the flavour of the Bark, as the infusion Experiment I. One dram of the chalybeate solution was added to this acid infusion. At first no change of colour took place, but in a few hours a slight black tinge appeared.

## EXPERIMENT IX.

Half an ounce of powdered Bark was well triturated, in the manner described in Experiment IV. with six ounces of warm water, after which the mixture was poured into a bottle, placed near a fire, and frequently shaken up. This process lasted forty-eight hours. The infusion, when strained off, was found to be more perfectly impregnated with the Bark, than the triturated infusion in cold water, Experiment IV. as appeared by comparing their colour, taste and smell, and by the deeper black which it instantly assumed on the admixture of one dram of the solution of *sal martis*.

## EXPERIMENT X.

Half an ounce of powdered Bark, and two drams of stone quick-lime, warm from the kiln, were  
 Vol. LVII. G g rubbed

rubbed together till they were thoroughly united; then 6 ounces of spring water were gradually poured on, the powder and the water were well incorporated by triture, and the mixture was set by to infuse for twelve hours. Two ounces of it were then filtered through a double linnen cloth, the remainder stood thirty-six hours longer; and was frequently shaken up, after which it was strained off. The smell of the Bark was almost entirely covered in both the infusions, which were strongly impregnated with the lime; and had an extremely disagreeable flavour. The first was of a pale colour, and possessed but a slight degree of bitterness; the latter had a deeper tinge, and was equally bitter and nauseous. Neither of them struck a black colour with the chalybeate solution, which, as soon as it was added, occasioned a yellow sediment, that in a few hours subsided to the bottom of the glass. Compared with the triturated infusion Experiment IV. these preparations appeared to be much weaker both in colour and taste. The residuum did not sensibly effervesce with oil of vitriol.

#### EXPERIMENT XI.

The decoctions and infusions were found to be impaired in strength, after standing six or seven days, though it was the winter season, and the weather was severely cold; the infusions became paler coloured, and at the same time deposited a slimy sediment. The decoction, at the end of seven days, assumed almost a milky hue, and struck but a faint black with the chalybeate solution. The simple infusion also

had lost much of its astringency; but the two triturated infusions were very little altered in that respect.

*Remarks on the preceding Experiments.*

PHYSICIANS in general agree, that the Peruvian Bark is most powerful in its effects, when taken in substance. But as the stomach is frequently unable to bear it, and as many patients have almost an invincible aversion to it in that form; it is of importance to determine in what preparations the virtues of this valuable drug are least impaired; and whether it may not be administered, under a form that is elegant, palatable, and at the same time sufficiently efficacious. The decoction of the Bark, has always appeared to me an injudicious preparation, for, though the cortex is not a substance of much volatility, yet there is a certain aroma accompanying it, which the heat of boiling water cannot fail to dissipate; and consequently the medicine is deprived of one of its component parts, in which probably some of its virtues reside. The Bark likewise undergoes a decomposition by boiling, the resin is separated from the gum, and remains suspended in the watery menstruum. This renders its appearance inelegant, its taste nauseous; and, I should apprehend, must considerably diminish its efficacy. For as the virtues of the Bark are strongest in its native state, they depend in all probability on its composition as a mixt; and must of course be impaired, by the disuniting of its constituent principles. By the first, second, and third, Experiments, it appears, that the

cortex yields its virtues at least as perfectly to cold, as to boiling water; and the simple infusion hath certainly many advantages over the decoction. It is a much more agreeable and elegant preparation, and the principles of the Bark remain perfectly unaltered in it, retaining the same proportions to each other, as in the substance of the drug itself. Nature hath so accurately combined and blended together the gummy and resinous parts of the cortex, that by their union they become soluble in menstrua, with which when separate they refuse to unite. Thus they reciprocally promote the solution of each other, in water and ardent spirits; and both the tincture and infusion are found, by Experiment, to be strongly impregnated with these two constituent principles of the Bark. The tincture is, without doubt, an elegant and palatable preparation; but it is liable to this objection, which indeed hold equally true against spirituous tinctures in general: that a sufficient dose of the medicine cannot be taken, on account of the heating nature of its vehicle. In low nervous fevers, hysterical disorders, and other cases where it is necessary to join cordials with the Bark, an infusion of it in red port may be prescribed with advantage. Under this form the empiric Talbot used to administer the cortex, in the paroxysms of intermittents; and so successful was his practice, that Lewis the XIVth was induced to purchase, at a large price, the secret of his specific. Orange peel is an useful ingredient, in preparations of the Bark; it gives a grateful warmth to the infusion, and adds, I think, considerably to its efficacy. The following formula is agreeable to the taste,

taste, and well adapted to a weak and delicate stomach.

℞ Pulv. Cort. Peruv. ℥j  
 Cort. Aurant. incif. ℥ss  
 Aq. Cinnam. ten. ℥xij  
 Aq. Cinnam. spir. ℥ij M. f.

Infusio in vase clauso, sine calore, per sesquidiem,  
 deinde coletur.

The use of trituration, in promoting the powers of solution, is evident from Experiments IV. VI. and VII. and would have been still more so, if a proper apparatus had been employed. The Count de la Garaye, a French gentleman, who is distinguished for his assiduity in applying the different branches of philosophy to the improvement of medicine, has described a very convenient machine; and pointed out an admirable process for obtaining from vegetables, by triture with water, the matters in which their virtues chiefly reside. The contrivance is extremely simple, consisting only of a vessel, to which a churning staff is fitted, which, by means of a wheel, and a cord, is perpetually whirled round with a rotatory motion. By this constant agitation, the most accurate diffusion is produced; and different portions of the menstrua are in quick succession applied to every part of the solvent.

From the Vth Experiment, no certain conclusions can be deduced; except that the extract is a much weaker preparation than is commonly supposed. It is liable to all the objections, which have been advanced against the decoction; with this additional one, that it

it is hardly possible to make it, according to the process of the London dispensatory, without giving it some degree of empyreuma. The extract employed in my experiment was prepared by a very diligent and careful apothecary; yet a considerable portion of it presently subsided in a powdery form to the bottom of the glass, which on examination appeared to be the burnt parts of the Bark. How little then is this officinal preparation to be depended upon, when we consider the carelessness and inaccuracy of the generality of apothecaries!

It is the practice of the most eminent physicians, to join acids with the Bark, in the cure of putrid diseases; and Sir John Pringle has observed, that in bilious fevers the cortex answered best in rhenish wine, after standing a night in infusion<sup>a</sup>. This suggested to me the VIIIth Experiment and I flattered myself, that by macerating the Bark in a mixture of vinegar and water, these two antiseptic medicines would be more accurately combined together; and that perhaps the acid might promote the dissolving power of the aqueous menstruum. In the latter expectation it appears that I was disappointed; and whether the former was better grounded, must be left to abler judges to determine. The result of this Experiment was so contrary to my expectations, that I determined to make further trials of the effects of vinegar in destroying that property, in certain vegetable substances, by which they strike a black colour with chalybeates, which has been long regarded as an almost indubitable test of astringency.

<sup>a</sup> Diseases of the Army, Edit. iv. p. 213.

An ounce of the infusion of chamomile flowers, was divided into two equal parts; to one was added a dram of white wine vinegar, to the other an equal quantity of spring water. Thus, with respect to dilution, they were precisely in the same circumstances; a tea spoonful of the *sal martis*, was then mixed with each of them, the portion which contained the vinegar, suffered no change of colour; the other instantly assumed a dusky hue. The same Experiment was repeated, with a very strong triturated infusion of the Bark; and the result was still more singular and curious. As soon as a dram of vinegar was added to half an ounce of the infusion, it changed the colour of it, from a deep and reddish brown, to a bright yellow; whilst the same quantity of water had no sensible effect on the other portion with which it was mixed. The chalybeate solution, as in the former Experiment, was then added. It produced no alteration in the portion with vinegar, but the other was changed into a perfect ink.

That moderate heat promotes and assists the action of water, as a menstruum on the Bark, is evident from the IXth Experiment; and it would be of advantage to determine, what degree of heat this drug will admit of, without suffering a decomposition. It should however be remarked, that this infusion though stronger, had neither so agreeable a flavour, nor was so sensibly impregnated with the aroma of the Bark, as the two made with cold water.

In an essay on the dissolvent power of quick-lime, a very ingenious chemist has observed, that all resinous bodies become soluble in water, when the cohesion of their particles is destroyed, by withdrawing the

the fixt air which they contain. This method of solution he endeavours to apply to many valuable purposes in medicine, and hath described several useful and curious processes for obtaining strong and elegant tinctures of the most active drugs, by means of quick-lime. The first part of the Xth Experiment, *mutatis mutandis*, was borrowed from him; and it was hoped, that an efficacious and palatable infusion might with tolerable expedition be made, by the process which he has laid down. But the success of my Experiment was by no means answerable to the plausibility and ingenuity of the theory, which induced me to attempt it. The infusion, after standing twelve hours, the time prescribed by Mr. Mc'Bride, was but weakly impregnated with the Bark; and when the maceration had been continued forty-eight hours, it by no means equalled in strength the preparation described Experiment IV. It appears therefore that quick-lime, whatever its effects may be upon other medicines, neither quickens nor increases the solubility of Bark in water; and it communicates to the infusion a taste which is intolerably nauseous and disagreeable. That the chalybeate solution should produce no change in the colour of these preparations, is agreeable to the laws of elective attraction. For the acid of vitriol, having a stronger affinity with absorbent earths than with metallic substances, forsakes the iron with which it was combined, and unites itself to the quick-lime. Hence arose the yellow ochry sediment, taken notice of in the Experiment. As the residuum, after filtration, did not effervesce with oil of vitriol, it is evident that quick-



quick-lime is not endued with the power of abstracting from Bark the fixed air which it contains.

Experiment X. furnishes no other inference than this obvious one, that the decoction and infusion of the Bark are calculated only for immediate use. The cortex is a substance of a very fermentable nature, as appears from the Experiments of Mr. M'Bride; and when its active parts are diffused in water, and separated from those which are merely ligneous and inert, it is not to be wondered at, that it undergoes those changes, to which all vegetables, when favourably circumstanced, are liable.

Warrington,  
February 1, 1767.

Thomas Percival.

Received April 24, 1767.

**XXVII.** *An Inquiry into the probable Parallax, and Magnitude of the fixed Stars, from the Quantity of Light which they afford us, and the particular Circumstances of their Situation, by the Rev. John Michell, B. D. F. R. S.*

~~Read May 7, and 14, 1767.~~ **T**HOUGH no man can at present doubt, that the want of a sensible parallax in the fixed stars, is owing to their immense distance, yet it may not perhaps be disagreeable to see, that this distance is farther confirmed by other circumstances; for let us suppose them to be, at a medium, equal in magnitude and natural brightness to the sun, to which they seem in all respects to be analagous. And, having laid this down as a foundation to build upon, let us inquire what would be the parallax of the sun, if he were to be removed so far from us, as to make the quantity of the light, which we should then receive from him, no more than equal to that of the fixed stars. In order to do this with accuracy, it would be proper to compare the quantity of light; which we at present receive from him, with that of the fixed stars, by some such methods, as are made use of by Monsieur Bouguer

Bouguer in his *Traité d'Optique* <sup>a</sup>; but as my present purpose does not require any such exactness, I shall deduce it in a more gross way from facts already well known. I shall assume Saturn then in opposition, exclusively of his ring (and when the earth and he are at their mean distances from the Sun) as equal or nearly equal in light to the most luminous fixed star. Now the mean distance of Saturn from the Sun, being equal to about 2082 of the Sun's semidiameters, the density of the Sun's light, at Saturn, will consequently be less than at his own surface, in the proportion of the square of 2082 (or 4334724) to 1. If Saturn therefore was to reflect all the light, that falls upon him, he would be less luminous in the same proportion; but, besides this difference in his brightness, his apparent diameter, in opposition, is at most but 105th part of that of the Sun, and consequently the quantity of light, which we receive from him, must again be diminished in the proportion of the square of 105 (or 11025) to 1. If we multiply these two numbers together, we shall have the whole of the light of the Sun to that of Saturn (upon the supposition of his reflecting all the light, that falls upon him) as the square of nearly 220000 (or 48,400,000,000) to 1; and removing the Sun to 220000 times his present distance, he would still appear at least as bright as Saturn, and his whole parallax upon the diameter of the earth's orbit would be less than two seconds. This must consequently be assumed for the parallax of the brightest of the fixed stars, upon the supposition that their light does not exceed that of Saturn.

<sup>a</sup> This work was published at Paris in 1760.

By a like computation we shall find, that the distance, at which the Sun would afford us as much light, as we receive from Jupiter, is not less than 46000 times his present distance, and his whole parallax, in that case, upon the diameter of the earth's orbit, would not be more than nine seconds, the light of Jupiter and Saturn, as seen from the Earth, being in the ratio of about 22 to 1, when they are both in opposition, and supposing them to reflect equally in proportion to the whole of the light that falls upon them.

But if Jupiter and Saturn, instead of reflecting the whole of the light, that falls upon them, should in fact reflect only a part of it, as for example, only a fourth or sixth (and this may very possibly be the case), we must then increase the distances computed above, in the proportion of 2 or  $2\frac{1}{2}$  to 1, to make the Sun's light no more than equal to theirs; and his parallax would be less, in the same proportion, than those already mentioned <sup>a</sup>.

Upon the supposition then, that the fixed stars are of the same magnitude and brightness with the Sun, it is no wonder, that their parallax should have hitherto escaped observation, since, if this is the case, it could hardly amount to two seconds, and probably

<sup>a</sup> The light, which we receive from the full Moon (according to Monsieur Bouguer's experiments in the work above-mentioned), is only a 300000th part of that which we receive from the Sun, whereas it ought to amount to no less than a 45000th part of it, according to the principles, which we have made use of in computing the quantity of light derived from Jupiter and Saturn; so that the Moon, as appears from these experiments, reflects no more than between a sixth and a seventh part of the light that falls upon her.

not more than one in Sirius himself; though he had been placed in the pole <sup>b</sup> of the ecliptic, and in those, that appear much less luminous, such for example as  $\gamma$  draconis, which is only of the third magnitude, it could hardly be expected to be sensible with such instruments as have hitherto been used in search of it.

We have assumed the magnitude of the fixed stars, as well as their brightness, to be equal to those of the sun; it is however probable, that there may be a very great difference amongst them in both these respects; and how much soever we may therefore be wide of the truth, in attempting to fix the distance of particular stars from this reasoning, yet there is a very great probability, that their mean distances, settled by this method, will not be much out, some exceeding and some falling short of it. And perhaps the consideration, that a star must be a thousand times as great, *cæteris paribus*, to appear equally bright, if it is placed at ten times the distance, may serve to make it probable, that the limits of the errors, which we are likely to commit, in judging by such a rule, are not so great as we might otherwise imagine them to be.

With regard to the difference there may be in the native brightness of different stars, though it is probably very considerable, yet I think we can hardly suppose, that it is equal to their difference in magnitude, at least if we except those, which are subject to certain changes, and which for that reason we may suppose to be luminous in some parts of their surfaces.

<sup>b</sup> The latitude of Sirius being only  $39^{\circ} 33'$ , his parallax will be a little less than two thirds of the whole parallax.

only. In other instances we may perhaps judge in some degree of the native brightness of different stars with respect to one another by their colour; those, which afford the whitest light, being probably the most luminous <sup>a</sup>.

<sup>a</sup> We have at present no means of judging of the comparative brightness of the Sun and of the fixed stars, in proportion to their respective sizes, excepting from the comparison of the Sun's brightness with that of our common fires; but the Sun's light exceeds the light of our brightest fires in so very great a proportion, (viz. of some thousands to one) that we want some middle terms to be able to form any analogy, which might serve to carry us farther. We find however in general, that those fires, which produce the whitest light, are much the brightest, and that the Sun, which produces a whiter light than any fires we commonly make, vastly exceeds them all in brightness; it is not therefore improbable, from this general analogy, that those stars, which exceed the Sun in the whiteness of their light, may also exceed him in their native brightness; now this is the case with regard to many of them; and, on the contrary, there are some that are of a redder colour.

If however it should hereafter be found, that any of the stars have others revolving about them (for no satellites shining by a borrowed light could possibly be visible), we should then have the means of discovering the proportion between the light of the Sun, and the light of those stars, relatively to their respective quantities of matter; for in this case, the times of the revolutions, and the greatest apparent elongations of those stars, that revolved about the others as satellites, being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us: and the actual quantity of matter which they contained would be known, whenever their distance was known, being greater or less in the proportion of the cube of that distance. Hence, supposing them to be of the same density with the Sun, the proportion of the brightness of their surfaces, compared with that of the Sun, would be known from the comparison of the whole of the light which we receive from them, with that which we receive from the Sun; but, if they should happen to be either of greater or less density than the Sun, the whole of their light not being

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As far then as we can guess at the parallax of the fixed stars from the principles above laid down, we may reasonably expect, that it should be exceedingly small even in those of the first magnitude; yet, besides the probability, that some of them may be either less, or less luminous than the Sun, it is not so small as to leave us altogether without hopes, that we may some time or other be able to discover it in some of them; for I think it is not impracticable to construct instruments, capable of distinguishing even to the 20th part of a second, provided the air will admit of that degree of exactness; but such instruments must be upon a plan a good deal different from those hitherto made use of, as they would otherwise be not only vastly too expensive, but also much too great and unweildy to be of any use.

But whatever room there may be to hope, that we may some time or other be able to discover the parallax of a few amongst the fixed stars, yet at the same time it seems probable, that we shall never be able to discover any sensible magnitude in their apparent diameters, which in Sirius himself, if he is not of less native brightness than the Sun, must be considerably less, whatever be his parallax, than the

affected by these suppositions, their surfaces would indeed be more or less luminous, accordingly as they were, upon this account, less or greater; but the quantity of light, corresponding to the same quantity of matter, would still remain the same.

The apparent distances, at which satellites would revolve about any stars, would be equal to the semianual parallaxes of those stars, seen from planets revolving about the Sun, in the same periodical times with themselves, supposing the parallaxes to be such, as they would be, if the stars were of the same size and density with the Sun.

hundredth, probably than the two hundredth part of a second; so that it would scarcely be distinguishable with a telescope, upon the former supposition, that should magnify six, or, upon the latter with one, that should magnify twelve thousand times.

Nor can we well expect to find their apparent diameters from any occultation by the moon, since a diameter of the hundredth part of a second would be covered by the moon, if it entered directly, in less than the fiftieth part of a second of time, and therefore a star can hardly enter so obliquely, as to appear to vanish by degrees; no star probably, which the moon can pass over, subtending an angle half so great. A star might however appear to vanish by degrees in an occultation by the planet Venus, especially if the occultation was to happen only a little before or after either station; but this is an event, which can occur so very seldom, that little is to be expected from it; and if Venus should be surrounded with an atmosphere, which is probably the case, it might very possibly then be of no service at all. For the same reasons also it is probable, that nothing can be determined from occultations by any of the rest of the planets, which upon other accounts are still less proper for the purpose than Venus.

There seems to be little chance therefore of discovering with certainty the real size of any of the fixed stars, and we must consequently be contented to deduce it from their parallax (if that should ever be found) and the quantity of light which they afford us, compared with that of the Sun. And in the mean time, till this parallax can be found, or something else may arise to furnish us with a more general analogy,



we can only suppose them, at a medium, to be equal in size to the Sun, this being the best means, which we have at present of forming some probable conjecture concerning the extent of the visible universe. That we may be the better enabled to do this, it seems to be an object worth the attention of Astronomers, to enquire into the exact quantity of light, which each star affords us separately, when compared with the Sun; that, instead of distributing them, as has hitherto been done, into a few ill defined classes, they may be ranked with precision both according to their respective brightness, and the exact degree of it.

A catalogue of the stars formed upon this plan, would answer several good purposes; for, besides giving us a more just and certain notion of their general distances, it would perhaps help us, especially if the parallax of a few amongst them should be discovered likewise, to trace some analogies in their situation, which might enable us to determine both their distances, and other circumstances relating to them, with still more probability; and it would be a standing register, by which future Astronomers might inform themselves of many variations, of which we are now ignorant for want of an ancient register of that kind.

From what has been said above, I think it is very probable, that we shall not be a great way from the truth in estimating the whole parallax of Sirius at one second, supposing him to be of the same size and native brightness with the Sun; this therefore I shall assume as a standard, till some better experiments shall inform us more exactly of the quantity of his

light. Now, according to the best judgment I have been able to make from some gross experiments, the quantity of light which we receive from Sirius does not exceed the light which we receive from the least fixed stars of the sixth magnitude in a greater proportion than that of 1000 to 1, nor in a less proportion than that of 400 to 1; and the smaller stars of the second magnitude seem to be about a mean proportional between the two. Hence the whole parallax of the least fixed stars of the sixth magnitude, supposing them of the same size and native brightness with the Sun, should be from about  $2''$  to  $3''$ , and their distance from about eight to twelve million times that of the Sun: and the parallax of the smaller stars of the second magnitude, upon the same supposition, should be about  $12''$ , and their distance about two million times that of the Sun.

I have hitherto argued about the distances of the fixed stars, upon the supposition of their being of the same size and brightness with the Sun; and, if this was really so, those which appear the brightest must be the nearest to us. That this is in general the case, I suppose, will be very readily allowed; for, though it is true, that a much greater degree of real magnitude may compensate for the greatness of distance, and there is no reason for assigning any one limit to them rather than another; yet, when it is as likely that the largest stars should be in any one part of space as in any other, the probability in favour of this hypothesis is very great: the real motions also, which have been observed amongst several of the brightest of the fixed stars, is another argument to the same purpose; and we shall find it still farther confirmed.

confirmed by very strong arguments of analogy drawn from the circumstances of the particular situation of the stars in the heavens.

It has always been usual with Astronomers to dispose the fixed stars into constellations: this has been done for the sake of remembering and distinguishing them, and therefore it has in general been done merely arbitrarily, and with this view only; nature herself however seems to have distinguished them into groups. What I mean is, that, from the apparent situation of the stars in the heavens, there is the highest probability, that, either by the original act of the Creator, or in consequence of some general law (such perhaps as gravity) they are collected together in great numbers in some parts of space, whilst in others there are either few or none.

The argument, I intend to make use of, in order to prove this, is of that kind, which infers either design, or some general law, from a general analogy, and the greatness of the odds against things having been in the present situation, if it was not owing to some such cause.

Let us then examine what it is probable would have been the least apparent distance of any two or more stars, any where in the whole heavens, upon the supposition that they had been scattered by mere chance, as it might happen. Now it is manifest, upon this supposition, that every star being as likely to be in any one situation as another, the probability, that any one particular star should happen to be within a certain distance (as for example one degree) of any other given star, would be represented (according to the common way of computing chances) by a fraction,

whose numerator would be to it's denominator, as a circle of one degree radius, to a circle, whose radius is the diameter of a great circle (this last quantity being equal to the whole surface of the sphere) that is, by the fraction  $\frac{60'}{6875.5'}$ , or, reducing it to a

decimal form, .000076154 (that is, about 1 in 13131); and the complement of this to unity, viz. .999923846,

or the fraction  $\frac{13130}{13131}$ , will represent the probability

that it would not be so. But, because there is the same chance for any one star to be within the distance of one degree from any given star, as for every other, multiplying this fraction into itself as many times as shall be equivalent to the whole number of stars, of not less brightness than those in question, and putting  $n$  for this number,  $\frac{13130^n}{13131^n}$ , or the fraction

$\frac{13130^n}{13131^n}$  will represent the probability, that no one of

the whole number of stars  $n$  would be within one degree from the proposed given star; and the complement of this quantity to unity will represent the probability, that there would be some one star or more, out of the whole number  $n$ , within the distance of one degree from the given star. And farther, because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars  $n$  might as well have been taken for the given star as any other, we must again repeat the last found chance  $n$  times, and consequently

consequently the number  $\frac{.999923846^n}{13130^n}$ , or the fraction  $\frac{13130^n}{13131^n}$  will represent the probability, that no where, in the whole heavens, any two stars, amongst those in question, would be within the distance of one degree from each other; and the complement of this quantity to unity will represent the probability of the contrary.

By a like reasoning, if we would compute the probability, upon the same supposition, that no two stars should be, one within the given distance  $x$ , and the other within the given distance  $z$  of some one particular star, we must, first, find the probability, that no star, of the whole number of stars  $n$ , would be within the distance  $x$  from the given star, which will

be represented, as before, by the fraction  $\frac{6875.5'^2 - xx'^2}{6875.5'^2}$

and, secondly, we must find the probability, that no star, of the whole number of stars  $n$ , would be within the distance  $z$  from the given star, which, for the same reason, will be represented by the fraction

$\frac{6875.5'^2 - zz'^2}{6875.5'^2}$ ; and the complements of these to unity

will represent the respective probabilities of the contrary: but the probability that two events shall both happen, is the product of the respective probabilities of those two events multiplied together; if therefore we multiply the two last mentioned complements together, we shall have the probability, that some two stars would be, one within the distance  $x$ , and the other

other within the distance  $z$  from the given star; and the complement of this to unity, will be the probability, that it would not be so: let us now suppose  $\frac{c}{d}$  to represent this last quantity, and, because the same event may as well happen in respect to any one star, as any other, multiplying this quantity into itself  $n$  times, according to the number of the stars, we shall have  $\left[\frac{c}{d}\right]^n$  representing the probability, that no where, in the whole heavens, would be found any two stars, one within the distance  $z$ , and the other within the distance  $z$  from the same star.

If now we compute, according to the principles above laid down, what the probability is, that no two stars, in the whole heavens, should have been within so small a distance from each other, as the two stars  $\beta$  Capricorni, to which I shall suppose about 230 stars only to be equal in brightness, we shall find it to be about 80 to 1.

For an example, where more than two stars are concerned, we may take the six brightest of the Pleiades, and, supposing the whole number of those stars, which are equal in splendor to the faintest of these, to be about 1500, we shall find the odds to be near 500000 to 1, that no six stars, out of that number, scattered at random, in the whole heavens, would be within so small a distance from each other, as the Pleiades are<sup>a</sup>.

<sup>a</sup> The computations of these probabilities are as follow.

The distance between the two stars  $\beta$  Capricorni is something less than  $3\frac{1}{2}$ ; according to the rule above laid down, therefore, if we suppose 230 stars equal to these in brightness, the proba-

If,

If, besides these examples that are obvious to the naked eye, we extend the same argument to the smaller

bility, that no two stars of that brightness will be found, any where in the whole heavens, within the distance of  $3' \frac{1}{3}$  from each

other, will be represented by the fraction  $\frac{6875.5^2 - 3.33 \&c.^2}{6875.5^2} \cdot 230 \times 230$ .

From twice the Log. of 6875.5 [7.6746086] then, subtract twice the Log. of 3.33 &c. [1.0457496] and the remainder 6.6288590 will be the Log. of the number of times, that  $\frac{6875.5^2}{3.33 \&c.^2}$  is contained

in  $\frac{6875.5^2}{3.33 \&c.^2}$  viz. 4254603 times, and consequently  $\frac{4254603}{4254603} \cdot 230 \times 230$

will be equivalent to the former fraction. From the Log. of 4254602, subtract the Log. of 4254603, and the remainder will be—.000000102, the proportional part for an unit in the number 4254603: this multiplied into 230 times 230, or 52900, gives—.0053958, the Log. of the whole quantity, which corresponds to the proportional part for an unit between 80 and 81; this quantity therefore is equivalent to the fraction  $\frac{80}{81}$  nearly, the complement of which to unity is  $\frac{1}{81}$ .

In the Pleiades, the five stars Taygeta, Electra, Merope, Alcyone, and Atlas are respectively at the distances of 11, 19  $\frac{1}{2}$ , 24  $\frac{1}{2}$ , 27, and 49 minutes from the star Maia nearly. From 7.6746086, twice the Log. of 6875.5, then, as before, subtract 2.0827854, twice the Log. of 11 (the number of minutes between Taygeta and Maia) and the remainder 5.5918232 will be the Log. of the number of times, that  $\frac{6875.5^2}{11^2}$  is contained in  $\frac{6875.5^2}{11^2}$  viz. 390682 times; and consequently a fraction, whose denominator is this number, and whose numerator is this number less by an unit, multiplied into itself 1500 times, will represent the probability, that no star out of 1500, scattered by chance in the whole heavens, would be within the distance of 11 minutes from the star Maia. From the Log. of 390681 therefore subtract the Log. of 390682, as in the former example, and the remainder will be—.00000111, the proportional part for an unit in the number 390682, which multiplied by 1500 will give us—.0016650 for the Log. of the probability sought. In like manner from stars,

stars, as well those that are collected together in clusters, such for example, as the Præsepe Cancri,

7.6746086 subtract 2.5800692, twice the Log. of  $19\frac{1}{2}$  (the number of minutes between Electra and Maia) and the remainder will be 5.0945394, the proportional part for an unit corresponding to the natural number of which will be—.00000349; and 1500 times this quantity, or—.0052350 will be the Log. of the quantity, representing the probability, that no star out of 1500 scattered by chance would be within the distance of  $19\frac{1}{2}$  minutes from Maia. If we follow the same rule for the three remaining stars Merope, Alcyone, and Atlas, we shall find the similar Logs. corresponding to these to be—.0076650, —.0100395, and —.0330300 respectively. The natural numbers corresponding to these five Logs. taken in the same order, are .996173, .988018, .982506, .977148, and .926766, which severally express the respective probabilities, that no stars out of 1500 scattered by chance, would be within the same distances, at which the five stars above mentioned are found to be, from Maia. The complements of these quantities to unity .003827, .011982, .017494, .022852, and .073234, which severally express the respective probabilities, on the contrary, that such stars would be found within the distances above specified from the star Maia, must all be multiplied together, to determine the probability, that these events should all take place at the same time. The sum of the Logs. of these numbers is  $-9 + .1279139$  or  $-8.8720861$ , which is therefore the Log. of all these numbers multiplied together; and subtracting this number from 0, or, what amounts to the same thing, changing the sign, we shall have the Log. of the number of times that this quantity is contained in unity, that is, about 744880000 times; a fraction, therefore, whose denominator is this number, and its numerator unity, will represent the probability in favour of all these events taking place together; and a fraction, whose denominator is the same number, and its numerator the same number less by an unit, will represent the probability of the contrary. But, as this event might as well have happened to any other star as to Maia, we must multiply this last fraction into itself 1500 times, according to the supposed number of stars, to find the probability, that it should not have happened any where in the whole heavens. Now the proportional part for an unit upon this number is—.000000005825, which multiplied the



the nebula in the hilt of Perseus's sword, &c. as to those stars, which appear double, treble, &c. when seen through telescopes, we shall find it still infinitely more conclusive, both in the particular instances, and in the general analogy, arising from the frequency of them.

We may from hence, therefore, with the highest probability conclude (the odds against the contrary opinion being many million millions to one) that the stars are really collected together in clusters in some places, where they form a kind of systems, whilst in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law or appointment of the Creator. And the natural conclusion from hence is, that it is highly probable in particular, and next to a certainty in general, that such double stars, &c. as appear to consist of two or more stars placed very near together, do really consist of stars placed near together, and under the influence of some general law, whenever the pro-

by 1500 gives us—.00000087377, the proportional part for an unit in somewhat more than 496000.

But it must be observed, that this number is smaller than it ought to be upon two accounts; for, in the first place, this method of computation gives only the probability, that no five stars would be within the distance above specified from a sixth, if they occupied the largest space, they possibly could do, under that limitation; and secondly, we have made no allowance upon account of the different magnitudes, which, if it had been attended to, would have given a somewhat greater result. These considerations, however, would have made the reasoning a good deal more intricate, and we have no need to descend to minutiae, a difference in the proportion of 10 to 1 not at all affecting the general conclusion.

bability is very great, that there would not have been any such stars so near together, if all those, that are not less bright than themselves, had been scattered at random through the whole heavens.

After what has been said, it will be natural to inquire, whether, if the stars are in general collected into systems, the Sun does not likewise make one of some system; and which are those, amongst the fixed stars, that belong to the same system with himself. Now, supposing the stars of one system to be, in general and at a medium, of the same size and brightness with those of another, the number of stars of any one apparent magnitude would bear the same proportion to the number of stars of any other apparent magnitude, as they would do, in case all the stars were scattered uniformly, and not in systems, provided the eye was not placed in or near to one of those systems. And, in this case, the brightness decreasing, as the square of the distance inversely, and the sphere, in which they are included, increasing, as the cube of the distance directly, the number of stars of any one degree of brightness and upwards, should be, as the cube of the square root of that brightness. Supposing then the faintest of the 2000 brightest stars to be less bright than the faintest of the first 70, in the proportion of about 30 to 1 (and I think the difference is not less than this) this number is smaller, than we might have expected, if the Sun was not one of a system, in the proportion of 2000 to about 12000 or 1 to 6; but I shall lay the less stress upon this argument, for want of a more certain determination of the proportion of light, which we receive from the stars of different magnitudes.

If,

If, however, upon a more accurate examination, it should be found, that the quantity of light above assigned is not far from the truth, or if the difference of light should be greater than I have supposed it to be (in which case the argument will be still stronger); this will add a considerable degree of weight to the other arguments drawn from analogy, in favour of the Sun's making one of a system of stars.

If we would now inquire, which are probably those stars, which compose part of the same system with the Sun; though it will not be possible to point them out with certainty, yet there are some marks, by which we may, with great probability include some and exclude others, whilst the rest remain more doubtful. Those stars which are found in clusters and surrounded with many others at a small distance from them, belong probably to other systems and not to ours. And those stars, which are surrounded with nebulæ, are probably only very great stars, which, upon account of their superior magnitude, are singly visible, whilst the others, which compose the remaining parts of the same system, are so small as to escape our sight. And those nebulæ, in which we can discover either none, or only a few stars, even with the assistance of the best telescopes, are probably systems, that are still more distant than the rest.

The Pleiades, as they appear to the naked eye, have been shewn above to be probably a system by themselves; and if we examine them still farther by means of telescopes, we shall find, that they are surrounded with so large a number of smaller stars, as to increase the odds against the contrary opinion many millions to one. Now supposing the Pleiades

to be in reality a system of stars, the probability is at least, I suppose, an hundred to one, that no one amongst them, of those visible to the naked eye, belongs to the same system with the Sun; but that these are only such stars, as are greater than the rest. The exact quantity of this probability depends upon the number of stars, visible to the naked eye, belonging to this system; the proportion, that the space occupied by the Pleiades bears to the whole heavens; and lastly, how far the situation of any one of the Pleiades falls in with the general analogy of the stars composing this system, if any such general analogy should appear.

As the nebulae, and smaller constellations, composed of a great number of stars, within a small distance from one another, belong probably to other systems; so those, which being placed at greater distances from each other compose the larger constellations, and such as have few or no smaller stars near them, when examined with telescopes, belong probably to our own system. Most of the stars of the first and second magnitude have this criterion to distinguish them as belonging to the same system with the Sun, besides several other circumstances, such as their greater brightness; the proper motions<sup>a</sup>; that have

<sup>a</sup> The apparent change of situation, that has been observed amongst a few of the stars, is a strong circumstance in favour of those stars being some of the nearest to us. This apparent change of situation may be owing either to the real motion of the stars themselves, or to that of the Sun, or partly to the one, and partly to the other. As far as it is owing to the latter (which it is by no means improbable may in some measure be the case) it may be considered as a kind of secular parallax, which, if the annual parallax of a few of the stars should some time or other be discovered, and the quantity and direction of the Sun's motion

been observed amongst some of them; their being more numerous, than we might naturally expect in proportion to the smaller stars, if they did not compose a part of the same system with ourselves, &c.

Besides the brighter stars, it is probable there are many of those of the smaller magnitudes also, that belong to the same system with the Sun. Now, if this should be the case, many of them being only fainter upon account of their less real magnitude, we may stand the same chance of finding a parallax amongst some of these, as amongst the brightest ones, provided we pitch upon such as happen to belong to our own system: to direct us with some probability to these, we have the circumstances above-mentioned of their composing larger constellations, and their having few stars lying very near to them; and perhaps there may be still a little more reason to suspect, that those stars form a part of the same system with ourselves, where, besides these circumstances, there have been observed changes of brightness, &c. amongst several of them in the same neighbourhood, such for instance as the changes, which have been observed amongst several of the stars in the constellations of the Swan, Cassiopeia, &c. We may, I think, also venture to add to these most of those stars, that are of a redder hue than the rest, as it is probable that they are a good deal bigger, in proportion to their brightness, than the whiter stars [see the last note:

should be discovered likewise, might serve to inform us of the distances of many of them, which it would be utterly impossible to find out by any other means.

but:

but two]. Many of them also have been observed to have a proper motion of their own, which with several other concurrent circumstances tends to make it highly probable, that they are some of the nearest to us.

Having thus endeavoured to establish the probability, that the Sun is one of a system of stars, placed by themselves in this part of the universe, I shall next inquire into some of the consequences of this hypothesis. Now if this is the case, and we suppose the whole number of stars, which belong to this system only (excluding those which belong to others), to amount to about 1000, we shall find it to be an even chance, that the parallax of the nearest amongst them does not exceed the parallax of one half that number in a greater proportion than that of 9 to 1, supposing the Earth to be placed in or near the centre of the whole system; nor in a greater proportion than that of about 12 to 1, supposing it to be placed very near the edge of the system; for supposing 1000 points to be placed within a sphere of any given magnitude, and that they are equally indifferent to every part of that sphere, if we assume any one of these points as a centre, we shall find, according to the known doctrine of chances, that there is an equal degree of probability whether any one of the rest shall or shall not fall within a sphere, described about the point assumed, of about seven ten thousandths of the size of the whole sphere; but the radius of such a sphere is about  $\frac{8}{1000}$  or a little less than  $\frac{1}{125}$  of the radius of the whole sphere, that is about  $\frac{1}{5}$  of the radius of a sphere of half the size of the greater one; and therefore a sphere, of about nine times this radius,

will

will include half the greater sphere, if its centre be assumed near the centre of the greater sphere, and a sphere of twelve times this radius will include half the greater sphere, if the point be assumed almost in the surface of it.

If we assume the stars belonging to our own system to be about 1000; since they are in all respects similar to the Sun (excepting perhaps such amongst them as are liable to frequent changes), and we have nothing to determine us to one magnitude rather than another, we may most reasonably suppose the magnitude of the Sun to be a medium amongst the whole number. Upon this supposition, he will probably rank only with the stars of the fourth magnitude; and his light therefore, if he was removed to the medium distance of the other stars (viz. a distance equal to the radius of the sphere, that would include half the stars of our own system) would hardly, I think, exceed a 200th part of the light of Sirius; and consequently, if the parallax of Sirius would be about one second, if he was of the same size and native brightness with the Sun, it will be an equal chance, that the parallax of one half of the stars, belonging to this system, is not less than one second divided by the square root of 200, that is a little more than  $4''$ ; and it will likewise be an equal chance, that the parallax of no one amongst them exceeds between 9 and 12 times that quantity, or a little more than two thirds of a second.

If, instead of 1000 stars, we suppose the whole number, belonging to this system, to be only about 350, the Sun will then, if he is of a medium size amongst them, rank probably with the stars of the third.

third magnitude, and his light, at a medium distance, upon this hypothesis, would be, I apprehend, about a 50th part of that of Sirius. And therefore, according to the reasoning above, we should then find it an equal chance, that the parallax of one half of these 350 stars would not be less than about  $8'' \frac{1}{2}$ ; and there would be the same chance, that the parallax of no one amongst them would be more than between  $50''$  and about  $1''$ .

In the former supposition of 1000 stars; the apparent magnitude of the Sun, when removed to the medium distance; &c. it seems not improbable, that the biggest star in the system may perhaps exceed the Sun, in the proportion of about 1000 to 1; and in the latter supposition of 350 stars, &c. that it may perhaps exceed the Sun, in the proportion of about 120 to 1.

In whatever proportion the diameter of the Sun is greater or less than the medium we have taken for it in the suppositions above, in the same proportion will the parallaxes be increased or diminished; and in the inverse triplicate of that proportion must their magnitudes be diminished or increased.

Let us now examine the circumstances of the Pleiades; and, assuming the respective distances of the stars, composing that system, from each other to be, at a medium, equal to those of our own, let us see what will be the consequences of this supposition. Now, if the Pleiades do not extend farther in the direction of a line drawn between the Earth and them, than in a direction at right angles to that line (which, from their composing a system, we have a right to suppose they do not) we can hardly allow the mean distance



distance of those, that are next to each other, amongst the fix stars visible to the naked eye, to be greater than what would subtend an angle, if seen directly from the earth, of about forty or fifty minutes. And consequently the distance between them and the earth would be about 70 times that distance, and their apparent brightness, seen from those that are next to each other, must be about 4900 times as great, as it appears to us ; But  $\gamma$  of the Pleiades, if I judge rightly, is not fainter than Sirius in a greater proportion than that of about 100 to 1 ; this star therefore must appear brighter to the nearest of those fix, which are visible to the naked eye, than Sirius does to us, in the proportion of about 49 to 1. Let us now suppose all the stars belonging to the Pleiades, as well those discoverable with telescopes, as those which are visible to the naked eye, to be contained within a sphere, whose apparent diameter at the earth is two degrees ; and consequently the mean distance of them from a spectator placed somewhere amongst them, as it might happen, would subtend an angle, when seen directly from the earth, of about a degree. Since therefore we have supposed the distances of the stars of our own system to be, at a medium, equal to those of the Pleiades, and consequently their mean distances from the earth to subtend at the Pleiades an angle of one degree, we shall have the distance of the Pleiades about 57 times as great as the mean distance of the stars of our own system, from the earth. Hence, if the biggest of the stars of our own system should be at this mean distance from us, and Sirius should be the biggest,  $\gamma$  of the Pleiades must exceed it in the proportion of about 200

to 1; for removing Sirius to 57 times his present distance, his light would then be fainter than it is, in the proportion of 3249 to 1, that is, fainter than  $\eta$  of the Pleiades in the proportion of 32.49 to 1, supposing  $\eta$  of the Pleiades, as above, to afford us a hundredth part of the light of Sirius; but the magnitude of stars, supposing them equally luminous and their distance to be given, is as the cube of the square root of their brightness; and therefore  $\eta$  of the Pleiades, upon this supposition, must be bigger than Sirius in the proportion of the cube of the square root of 32.49 (that is 185) to 1.

But I must observe, that according to general (and, I believe, I may say universal) analogy in all those nebulae, in which we discover stars bigger than the rest, these stars are placed towards the middle of their respective systems, and, if therefore the same thing obtains with regard to our own system, this will make  $\eta$  of the Pleiades still something greater.

If the distance of the Pleiades is greater than the mean distance of the stars of our own system, in the proportion of 57 to 1, it would be necessary, in order to make stars, of the same real magnitude amongst the Pleiades, equally visible to us with those of our own system, to take in a pencil of rays of a greater diameter, than the pupil of the eye, in the same proportion: this, after proper deductions for the loss of light, could not well be effected by an object lens of less than two feet aperture. Now Dr. Hooke tells us, in his *Micrographia*, that, with a telescope of twelve feet length, he discovered in the Pleiades 78 stars, and, with longer telescopes, many more; but

if a telescope of twelve feet length, the aperture of whose object lens was probably less than two inches, increased the number of visible stars in the Pleiades to 78, we may well suppose, that with an object lens of two feet diameter, they would amount to more than 1000. What this number would be must depend however upon the gradation of real magnitude amongst the stars of that system, to which there must necessarily be some limit, and it is not therefore improbable, that observations of the increase of the number of stars amongst the Pleiades, &c. with telescopes of larger apertures, especially if this was carried on to very large sizes, might serve to inform us of many circumstances, both with regard to this gradation, and perhaps some other things, that would enable us to judge with more probability concerning the distances, magnitudes, &c. of the stars of our own system.

If we now imagine a spectator amongst the Pleiades to take a view of this system from thence, supposing the distance, as before, 57 times the mean distance of our own stars, we should appear to him as a nebula, in which there would be no star bright enough to be distinguishable by the naked eye; and with a telescope, the aperture of whose object lens was two inches, he would hardly, I suppose, be able to distinguish more than half a score stars at the utmost.

Having hitherto supposed the distances of the stars of our own system to be the same with those of the Pleiades, and examined the appearances according to that hypothesis, let us now, instead of their distances, suppose their magnitudes to be the same. This would make this system, as seen from the Pleiades, to subtend an angle of about twelve degrees instead of two,

and about half a score of the biggest stars would be there visible to the naked eye; but a telescope, whose object lens was of two inches aperture, would in that case, I apprehend, take in almost all the stars belonging to this system of the fourth magnitude and upwards. These appearances fall in less, I think, with the general analogy of what we see in the heavens, than the former supposition; but for want of more observations I cannot say this with any certainty: in the mean time however till we have something farther to go upon, it may not perhaps be amiss to take a kind of medium between the two, and suppose the Pleiades to be at about twenty times the mean distance of the stars belonging to our own system, in which case  $\eta$  will exceed the biggest of these, in the proportion of about eight or ten to one; or it will exceed the Sun, according to our former suppositions of his being of a medium size amongst 1000 or 350 stars, in one case in the proportion of about eight or ten thousand, and in the other, about a thousand or twelve hundred to one; its parallax in the former case being about  $36''''$  and in the latter about  $1 \frac{1}{4}''''$ .

I shall conclude this inquiry with one observation more, concerning the appearance of the stars of our own system, as seen from great distances. Whatever then the real distance and magnitude of these stars may be, provided we have not been greatly out in assigning the proportion of their light in respect to that of the Sun and one another, if they were to be seen from a distance, at which the whole system would not subtend an angle of more than six or eight minutes, it would appear only as a nebula, no single star being visible with perhaps any telescope, that has ever yet

been made ; for at this distance, the distance between the earth and the biggest star of this system not subtending an angle of more than about three minutes (that is, about a twelve hundredth part of the radius) the stars of this system must appear less luminous than they do to ourselves, in the proportion of the square of 1200 (or 1440000) to 1. And supposing the light of Sirius to exceed that of the least visible star in the proportion of about 1200 to 1, the brightest star therefore would still require to have its light increased in the proportion of 1200 to 1, before it would begin to be distinguishable. To do this would require a telescope, that should take in a pencil of rays of a larger diameter than the pupil of the eye, in the proportion of 35 to 1, that is, a pencil of about a foot diameter, exclusive of deductions ; for the pupil of the eye is not less than a third of an inch in diameter, in a clear star-light night, when there is no Moon ; but to obtain such a pencil, we must not make use of a refracting telescope (with two lenses only) of less than 15 inches, nor a reflector of less than nearly two feet aperture. This may serve to shew us, that those nebulae, in which we cannot distinguish any stars, may yet reasonably be supposed to consist of stars, though too far distant to be singly visible ; since this would be the case with our own system, seen from as great a distance as we may well suppose those nebulae to be from us, if we judge of it from the magnitude of the visible area, which they occupy in the heavens.

*Of the twinkling of the fixed stars.*

Having never yet seen any solution of the twinkling of the fixed stars, with which I could rest satisfied,

fied<sup>a</sup>, I shall offer the following, which may not perhaps be found an inadequate cause of that appearance; at least it has undoubtedly some share in producing it, especially in the smaller stars.

It is not, I think, unreasonable to suppose, that a single particle of light is sufficient to make a sensible impression upon the organs of sight. Upon this supposition, a very few particles of light, arriving at the eye in a second of time, will be sufficient to make an object visible, perhaps not more than three or four; for though the impression may be considered as momentary, yet the perception, occasioned by it, is of a much longer duration: this sufficiently appears from the well-known experiment of a lighted body whirled round in a circle, which needs not make many revolutions in a second, to appear as one continued ring of fire. Hence then it is not improbable, that the number of the particles of light, which enter the eye in a second of time even from Sirius himself, may not exceed three or four thousand; and from stars of the second magnitude, they may therefore probably not much exceed an hundred. Now the apparent increase and diminution of the light, which we observe in the twinkling of the stars, seems to be repeated at not very unequal intervals, perhaps about four or five times in a second: why may we not then suppose, that the in-

<sup>a</sup> Some astronomers have lately adopted, as a solution of this appearance, the extreme minuteness of the apparent diameters of the fixed stars, which, they suppose, must, in consequence of this, be intercepted by every little mote, that floats in the air; but, that an object should be able to intercept a star from us, it must be large enough to exceed the apparent diameter of the star by the diameter of the pupil of the eye; so that, if the star was a mathematical point, it must still be equal in size to the pupil of the eye.

equalities, which will naturally arise from the chance of the rays coming sometimes a little denser and sometimes a little rarer, in so small a number of them as must fall upon the eye in the fourth or fifth part of a second, may be sufficient to account for this appearance? An addition of two or three particles of light, or perhaps of a single one upon twenty, especially if there should be an equal deficiency out of the next twenty, would I suppose be very sensible: this seems at least probable from the very great difference in the appearance of stars, whose light is much less different than, I imagine, people are in general aware of; the light of the middle-most star in the tail of the great Bear does not, I think, exceed the light of the very small star next to it, in a greater proportion than that of about sixteen or twenty to one; and Monsieur Bouger tells us, in his *Traité d'Optique* before-mentioned, that he finds a difference in the light of objects of one part in sixty-six sufficiently distinguishable.

It will perhaps be objected, that the rays coming from Sirius are too numerous to admit of a sufficient inequality, arising from the common effect of chance, so frequently as would be necessary to produce this effect, whatever might happen in respect to the smaller stars; but till we know what inequality is necessary to produce this effect, we can only guess at it either one way or the other; there is however another circumstance, that seems to concur in the twinkling of the stars, besides their brightness, and this is a change of colour. Now the red and blue rays being very much fewer, I apprehend, than those of the intermediate colours, and therefore much more liable to inequality from the common effect of chance, may help very much to account for this phenomenon, a small excess

or defect in either of these making a very sensible difference in the colour.

It will now naturally be asked, why the frequency of the changes of brightness should not be often much greater, as well as sometimes less, than that above-mentioned, and why the interval of the fourth or fifth, or some such part, should be pitched upon, rather than the fortieth or fiftieth part of a second, or than a whole second, &c. for, according to the length or shortness of the time assumed, the changes, that will naturally occur, from the effect of chance, will be smaller or greater in proportion to each other. The answer to this question will, I think, tend to render the above solution more probable, as well as to throw a good deal of light upon the whole subject. The lengths of the times then between the changes of brightness, if I am not mistaken, depend upon the duration of the perception before mentioned, occasioned by the impression of the light upon the eye, than which they seem to be neither much longer nor shorter. Whatever inequalities fall within a much shorter time than the continuance of this perception, will necessarily be blended together, and have no effect, but as they compose a part of the whole mass; but those inequalities, which fall in such a manner as that they may be assigned to intervals nearly equal to, or something greater than the continuance of this perception, will be so divided by the imagination, which will naturally follow, and pick them out as they arise.



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PHILOSOPHICAL TRANSACTIONS.

VOLUME LVII. PART II.

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PHILOSOPHICAL  
TRANSACTIONS,  
GIVING SOME  
ACCOUNT

OF THE  
Present Undertakings, Studies, *and* Labours,

OF THE  
INGENIOUS,

IN MANY  
Considerable Parts of the WORLD.

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VOL. LVII. PART II. For the Year 1767.

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L O N D O N :

Printed for L. DAVIS and C. REYMERS,  
Printers to the ROYAL SOCIETY,  
against *Gray's-Inn Gate*, in *Holbourn*.

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M.DCC.LXVIII.



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# PHILOSOPHICAL TRANSACTIONS. PART II.

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Received April 9, 1767.

XXVIII. *Thermometrical Observations at Derby, by Mr. John Whitehurst, communicated by Charles Morton, M. D. Sec. R. S.*

Dear Sir,

Read May 14, 1767. **W**E experienced a much greater degree of cold at Derby, in the late frost, than perhaps was ever observed in England; and the quick transitions were no less remarkable.

On Sunday the 18th of last month, at nine of the clock in the evening, my thermometer stood at 20. At half an hour after nine, nearly one degrew below 0. At seven the next morning, 30. external air.

I am, Sir,

Your most obedient servant,

Derby,  
Feb. 15, 1767.

John Whitehurst.

XXIX. *An Attempt to interpret the Legend and Inscription of a very curious Phœnician Medal, never hitherto explained. In a Letter to the Right Honourable the Earl of Morton, President of the Royal Society, from the Rev. John Swinton, B. D. F. R. S. Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

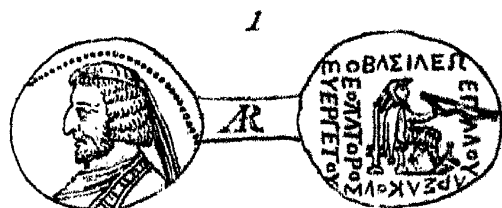
My Lord,

Read May 21, 1767. CAPT. Swinton some time since put into my hands a very curious silver coin, taken, as he informed me, out of your Lordship's valuable cabinet. He at the same time also imparted to me your Lordship's commands, relative to that coin. In obedience to which, I now do myself the honour to send you the following interpretation of the legend and inscription it exhibits. This, as I cannot help believing it true, or at least not very remote from truth, may possibly, I would flatter myself, prove not altogether unsatisfactory or unacceptable to your Lordship.

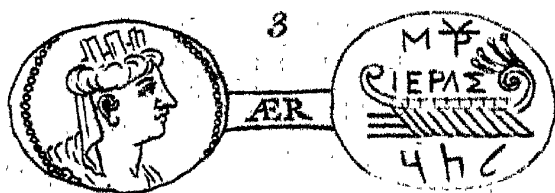
I.

On one side (see TAB. XII.) the medal presents to our view Jupiter sitting in a chair, with his eagle before him, a bunch of grapes in his right hand, and a sort of lance, or rather staff, as it should seem, in his left. Behind him the legend באל תרז, BAAL TARZ, or BAAL TARS, formed of Phœnician letters, may be discerned;



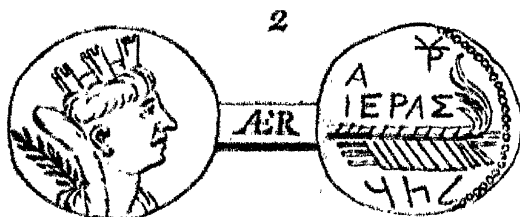


*Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.*

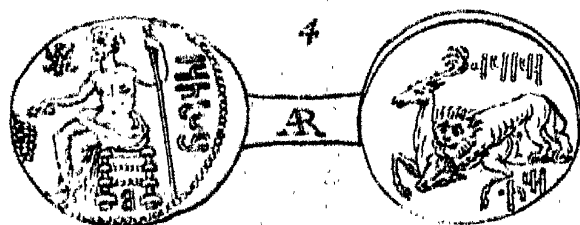


*Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.*





*Apud Joannem Swinton, S.T.B. Oxoniens. R.S.S.*



*È Theſauro Illuſtriſſimi Comitù de  
Morton, R.S.P.*



and the element B, inverted, is visible under the chair. On the other side we discover a lion seizing upon, or rather tearing, a stag; over and under which the two Phœnician words מוֹרַג מוֹת, MIZZERAG MOTH, or MUTH, in their proper characters, unless I am greatly mistaken, seem clearly to appear. The workmanship of all the figures, but particularly of the lion and the stag, is finished in a high manner, and exquisitely fine.

Several (1) similar medals have been published by Lord Pembroke, M. Morell, and M. Pellerin. Amongst these we find four in Lord Pembroke's noble cabinet, emitted from the same mint, and done in the same taste, with that I am here considering, three of (2) which exhibit a lion tearing a bull. Not one of the legends, or inscriptions, with which they are adorned, has been hitherto explained. A proper and accurate draught of the medal before me has never yet, as I apprehend, been given us, by any antiquary, or author whatsoever, and therefore the piece itself may pass for an inedited coin.

## II.

That the legend of this medal is formed of the letters *Beth, Ain, Lamed, Thau, Resch*, and *Zain*, from which result the words BAAL TARZ, or BAAL TARS, will, I flatter myself, not be denied by the learned. The first character strongly resembles the

(1) *Numism. Antiqu. Sc. à Thom. Pembr. et Mont. Gomer. Com. Collect. P. 2. T. 87. Morell. Specim. Univers. Rei Nummar. Antiqu. Sc. P. 245. Tab. xxvi. Lipsiæ, 1695. Peller. Recueil de Médailles de Peuples et de Villes, Sc. Tom. Trois. Pl. CXXII. A Paris, 1763.*

(2) *Numism. Antiqu. à Thom. Pembr. et Mont. Gomer. Com. Collect. ubi sup.*

*Beth* of the (3) Palmyrenes, which had most certainly the same origin with the Phœnician *Beth*. Nor can it be taken here for any other element. Besides, the common Phœnician form of that letter and the character in question are (4) promiscuously used on this species of coins. The second element is *Ain*, as appears from other (5) authentic monuments. The third is *Lamed*, though not so well preserved here as on some other similar (6) coins. The fourth, at first sight, will be pronounced *Thau*, by every one at all versed in this branch of literature. That the fifth is *Resch*, we may fairly presume; as the similar letter *Daleth* appears (7) open at the top, like this, on a medal of Sidon, published by M. Pellerin, and as such a figure of *Resch* actually presents itself to our view on the reverses of two of my Tyrian (8) coins. Nor does the correspondent character on another of these Phœnician medals, a draught of which has been given (9) us by M. Morell, recede much from the common Phœnician form of *Resch*, which on these pieces likewise sometimes occurs. The sixth is *Zain*, as has been (10) rendered elsewhere sufficiently evident. What the inverted B, under the chair, was intended to denote, unless we may consider it as the initial letter

(3) *Philos. Transact.* Vol. XLVIII. p. 693, 740.

(4) Peller. ubi sup. Pl. CXXII. p. 164.

(5) *Philos. Trans.* Vol. LIII. p. 282. & Vol. LIV. Tab. xxiv. Peller. *Recueil de Médail. de Peup. et de Vil. &c.* Tom. Trois. p. 164. Pl. CXXII. A Paris, 1763.

(6) Id. ibid.

(7) Peller. ubi sup. Tom. Sec. Pl. LXXXII. N. 21. p. 226.

(8) See Tab. XII. N. 2, 3.

(9) Morell. ubi sup. Peller. Tom. Trois. Pl. CXXII.

(10) *Philos. Trans.* Vol. XLVIII. p. 715. & Vol. LIII.

of the first word BAAL, it may perhaps be not so easy to decide.

From the two Phœnician proper names BAAL TARZ, or BAAL TARS, it seems highly probable, that the medal was struck at Tarsus, the capital of Cilicia, seated in a country abounding with wild beasts, (11) particularly lions and stags, and famous for the birth of the great apostle St. Paul. For BAAL TARZ, or BAAL TARS, is equivalent to JUPITER TARSENSIS, JUPITER OF TARSUS, or THE LORD OF TARSUS. So we find this Pagan divinity denominated (12) on coins ZETC KACIOC, JUPITER CASIUS, ZETC KAHITQAIETC ANTI-OXEON, JUPITER CAPITOLINUS ANTIOCHENSIIUM, ZETC CTPATHOC AMACTPIANON, JUPITER IMPERATOR AMASTRIANORUM, &c. And, upon (13) a coin of the emperor Hadrian, we read ΔΙΟC ΤΑΡΧΕΩΝ ΜΕΤΡΟΠΟΛΕΩC, JOVIS TARSENSIIUM METROPOLEOS, or JOVIS TARSENSIS, OF JUPITER OF TARSUS, the very appellation given this pretended deity by the medal now in view. Nay, he is there exhibited with his eagle, and sitting in a chair, as he appears upon the Phœnician coin I am endeavouring to explain. The notion here advanced is also strongly supported by two pieces of the emperor Gordian, published by Dr. Vaillant. One of these on the reverse presents to our view Bacchus, with a

(11) *Ælian. de Animal. Lib. V. c. lvi. Boet. Chan. Lib. I. c. v. p. 376, 377. Francof. ad Moen. 1681.*

(12) *Joan. Vaill. Numism. Imp. August. & Caesar. à Pop. Rom. Dir. Græcè loquent. &c. pass.* In like manner the great deity of Berytus went under the appellation of BAAL BERITH, according to Bochart. *Boch. ubi sup. p. 859.*

(13) *Joan. Vaill. ubi sup. p. 36. Amstelædami, 1700.*

bunch of grapes (14) in his right hand, as Jupiter appears on the medal before me, attended by the inscription ΤΑΡΧΟΥ ΜΕΤΡΟΠΟΛΕΩΣ, &c. ΤΑΡΣΙ ΜΕΤΡΟΠΟΛΕΩΣ, &c. and the other is adorned with the same inscription, and a lion tearing a (15) bull, the very symbol that occurs on three of Lord Pembroke's (16) coins, so perfectly similar to that which is the object of my attention here.

In farther eviſion of the point I am contending for, I muſt beg leave to remark, that two pieces of Tarſus with Jupiter Tarſenſis on the reverſe, in the very ſame attitude, and, in other reſpects, reſembling the figure of the ſame deity on the medal now conſidered, have been lately (17) publiſhed by M. Pellerin.

From the coin of Gordian (18) adorned with Bacchus on the reverſe, holding a bunch of grapes in his right hand, we may infer, not only that Bacchus was worſhiped at Tarſus, but likewiſe that wine was made in the diſtrict appertaining to that city. This will eaſily and naturally account for the grapes held by Jupiter, or Baal, in his right hand, on the piece I am attempting to illuſtrate here.

It muſt be farther remarked, that ſuch medals as this, as well as many others, are dug up in the neighbourhood (19) of Kephē, the Seleucia Pieria of the antients, which ſtood at no very great diſtance from

(14) Joan. Vaill. ubi ſup. p. 157.

(15) Id. Ibid.

(16) *Numism. Antiqu. &c.* à Thom. Pembr. et Mont. Gomer. Com. Collect, p. 2. T. 87.

(17) Peller. ubi ſup. Tom. Sec. Pl. LXXIV. p. 175—177. A Paris, 1763.

(18) Joan. Vaill. ubi ſup. p. 157.

(19) Pococke's *Deſcript. of the Eaſt*, Vol. II. p. 186.

mount Amanus and the borders of Cilicia. This, in conjunction with what has been already offered in support of my present opinion, will amount to the strongest presumption, if not an irrefragable proof, that the piece in question was struck at Tarsus. The late Mr. Arthur Pullinger shewed me two similar coins, found near the place just mentioned, which he purchased at Aleppo, and brought with him to England out of the East.

That the Phœnician name TARZ should have answered to the Greek ΤΑΡΣΟΣ, and the Latin TARSUS, can be no matter of surprize to any person at all versed in oriental literature. For 'tis too well known to stand in need of a proof, that ΟΣ is a pure Greek termination, and vs. a Latin one, superadded to the Phœnician word. And that the Greeks (20) not infrequently converted *Zain* into *Sigma*, as the Latins did into *S*, is a most obvious truth. Many instances of such conversions, in oriental names adopted by those nations, might, with the utmost facility, be produced.

### III.

With regard to the characters on the reverse, their powers seem sufficiently deducible from other monuments heretofore explained. The first and fifth will undoubtedly be taken for *Mem.* The second points plainly at *Zain*. The third will be looked upon either as *Daleth*, *Caph*, or *Resch*; though, if the sense be duly attended to, I think we must pro-

(20) Bochart. Chan. Lib. I. c. xxvii. p. 559. c. vi. p. 390. &c. alib. Francof. ad Moen. 1681. The Arabs, after the conquest of Syria, adopted the Greek, or Latin, name; which is evidently not so antient as the Phœnician [𐤕𐤒], TARZ, or TARS.

nounce it *Resch*. The fourth will be allowed to represent *Ghimel*, as that element appears in some of the (21) Palmyrene inscriptions, and particularly that found (22) at Teive. The sixth, as I apprehend, must pass for *Thau*. Now the words formed of these letters, מוֹרַג מוֹרַג, MIZZERAG MOTH, or MISSERAG MVTH, may be rendered AB IMPLECTENDO (COLLIGANDO, CONNECTENDO, &c.) MORS, or rather A CONNECTIONE (COLLIGATIONE, IMPLEXV, CONGRESSV, &c.) MORS, FROM THE CONNECTION (SEIZURE, ATTACK, &c.) comes DEATH. In Hebrew the second letter of the first word here is *Sin*, in Chaldee *Samech*, and in Arabic *Ze*. That in all these languages, however, the term is nearly the same, and has nearly the same signification annexed to it, we learn (23) from some of the best lexicographers.

Nor can any thing be more consonant to the nature of the symbol on the reverse than the sense here assigned the inscription. They may both perhaps be supposed to have alluded to the Indian manner of hunting stags with lions, mentioned by *Ælian* (24), which might formerly likewise have prevailed in Cilicia; or to the production of those (25) animals, which were anciently so numerous in that country; or lastly to some remarkable event, that not impro-

(21) *Philosoph. Transact.* Vol. XLVIII. p. 693, 740.

(22) See Vol. LVI. Tab. I. Fig. 1. p. 5.

(23) Val. Schind. Johan. Buxtorf. F. *Lex. Chald. Talm. & Rabbin.* Leonh. Reckenb. & Jo. Christ. Clod. *Lex. Hebr. Select.* Lipsiæ, 1744.

(24) *Ælian. de Animal.* Lib. XVII. c. xxvi. p. 990. *Coloniz Allobrogum*, 1616.

(25) *Ælian. de Animal.* Lib. V. c. lvi. Boet. Chan. Lib. I. c. v. p. 376, 377. *Francofurti ad Moenum*, 1681.



bably happened about the time the medal was struck.

## IV.

The last remark naturally leads me to the age of the coin I am endeavouring to explain, to which perhaps a pretty near approach may be made. I formerly published a similar (26) medal, that, unless I am greatly deceived, first appeared in some part of the Parthian territories. It presented to our view on one side Jupiter Tarsensis, as he is exhibited by the piece before me, and two words perfectly agreeing with those attending him on this piece, being formed of the very same Phœnician characters. On the other we perceived a human figure sitting in a chair, with an arrow in both its hands, before which stood a bow, a weapon not seldom occurring on the reverses of the Parthian coins (27). Hence it should seem,

(26) *Philosop. Transact.* Vol. LI. Tab. xvii. p. 683.

The Parthian kings sometimes wore a sort of long breeches, reaching down to their ancles, similar to what is exhibited by the coin here referred to, as we learn from the reverse of a Parthian medal now in my possession, a draught of which may be seen in (a) Tab. XII. N. 1. This will serve still farther to evince the truth of the point here contended for.

That the coin by me formerly published ought to be ranked among those struck in the Parthian territories, will be rendered still more probable by certain human figures that occur amongst the ruins of Persépolis; which are (b) adorned with a sort of cap, not a little similar to that on the head of the prince presented to our view by this coin. The bow and arrow likewise on it, (c) visible also on a very curious Daric, will confirm what has been here advanced, in support of my present opinion.

(a) *Philos. Transact.* Vol. LVII. Tab. XII. N. 1. p. 266.

(b) Engel. Kämpfer. *Asienat. Exotie.* &c. p. 311. Lemgoviz, 1712.

(c) Hyde *Hist. Relig. Veter. Persar.* &c. p. 113. Oxon. 1760.

that these pieces were struck at Tarsus, when the Parthians (28) were masters of Cilicia. Now I cannot learn from history, that they were ever possessed of that province before the year of the Julian Period 4673, which preceded about forty one years the birth of CHRIST, when (29) it was reduced by Labienus; nor after the following year, when they were overthrown, and expelled the Roman territories (30), by Ventidius. Perhaps

(28) That the piece before me is of Parthian original, seems likewise farther to appear from a complex figure representing a lion tearing (a) a bull, the very type on the reverses of several coins, mentioned in the beginning of this paper, and agreeing in almost all respects with this piece, to be met with in the ruins of (a) Persepolis. For that part of those ruins, at least, are Parthian remains of antiquity, I have (b) formerly observed. But should any one consider them as Persian monuments, and even as prior to the reign of Alexander the Great, yet still the point I would in some measure support by them will thereby be rendered probable, if not incontestable. For the Persians and the Parthians may be looked upon, with sufficient propriety, as one and the same nation; the same habits, the same customs and manners, and the same religion, seeming antiently to have prevailed amongst them.

Whenever therefore we find a lion tearing a bull, or a stag, on any Greek medals, we may conclude it not improbable, that the prince or people to whom they belonged adopted a type of Parthian, or Persian, extraction.

(c) Gesner assigns the coin I am considering a place amongst those of the Syrian kings, but knows not what to make of it. He only observes, that Morell takes it to be a Parthian medal, and that others entertain a different opinion; not attempting, in any part of his book, to oblige the learned world with an explication of it.

(29) Dio, Lib. xlviii. L. Flor. Lib. IV. c. ix. Plutarch. in *Anton. Appian. Alexandrin. in Syriac. & Parthic. & de Bell. Civil. Lib. v.*

(30) Dio, ubi sup. Appian. in *Parthic. Liv. Epit. Lib. cxxvii. L. Flor. Lib. IV. c. ix. Plutarch. in Anton.*

(a) Eng. Kæmpf, *Amœnitat. Exotic.* p. 339. Lengovia, 1712.

(b) *Philos. Transact.* Vol. XLIX. P. i. p. 598, 599, 600.

(c) *Gesn. Tab. Reg. Syr. IX. N. 2. Tigusi, 1738.*

therefore

therefore we may be permitted to suppose our medal to have been emitted from the mint at Tarsus in one of those years. If this should be admitted by the learned, and I can see nothing unreasonable in such a supposition, we may naturally enough account both for the symbol and the inscription. For the Parthians at this time had the Romans in such contempt, by reason of their former victories over them, that a body of their forces advancing to the relief (31) of Labienus, then incamped on Mount Taurus, at no very great distance from them, engaged a Roman army, before their junction with that general. Being therefore overthrown in this battle by Ventidius, (32) who commanded the Romans, and most of them cut in pieces, they received the reward justly due to their temerity and presumption. This certainly gives us reason to believe, that the symbol and inscription I am now upon pointed at the victories gained by the Parthians over the Romans, about the time above mentioned; and that they were intended to perpetuate the memory of those victories, to the remotest periods of time.

In farther support of what has been here advanced, it may not be improper to observe, that a similar (33) medal published by M. Pellerin seems to me to be undoubtedly Parthian. On one side we discover Hercules, with a club in his right hand, and a lion's skin in his left, as he appears on a coin of Tarsus (34), together with a bow, a symbol not

(31) Dio, ubi sup.

(32) Idem ibid.

(33) Peller. ubi sup. Tom. Trois. Pl. CXXII. II.

(34) Joan. Vaill. ubi sup. p. 157.

feldom visible on the Parthian coins. The reverse agrees with that of the piece I am considering, in every particular but the inscription; from which, tho' Phœnician, that of the medal before me is intirely different. That on M. Pellerin's piece, however, is formed of six letters; the three first of which are apparently *Mem*, *Lamed*, *Caph*, which constitute the word מֶלֶךְ, MELEC, KING. The fourth greatly resembles the Chaldee *Vau*, and not a little one of the forms of the same element (35) used by the Palmyrenes. The fifth must be taken, as I apprehend, for *Resch*, being very like the figure of that letter on the medal engaging my attention here. The sixth may pass for a form of *Dalet*, that has suffered from the injuries of time. Admit this, and I see not the least impropriety in such a concession, and the word may be read OROD; which, stripped of the Greek termination, is not only a Parthian proper name, but the very name of the prince who sat upon the throne when (36) the Parthians were possessed of Cilicia, and their forces overthrown by Ventidius. Had it not been for the suppression of the latter *Vau*, this word would have been intirely the same with VOROD, or OROD, exhibited by three Palmyrene (37) inscriptions, and written ΤΟΡΟΔΗΣ, VORODES, or ORODES, in the correspondent Greek ones. And with regard to the suppression of *Vau*, this was by

(35) *Philosoph. Transact.* Vol. XLVIII. p. 693. & Tab. XXX. Num. 3.

(36) Appian. Alexandrin. & Dio, ubi sup.

(37) *Philosoph. Transact.* Vol. XLVIII. p. 694, 695.

no means uncommon amongst the Phœnicians, as I have elsewhere (38) incontestably proved.

If the interpretation of this short inscription now laid down should meet with the approbation of the learned, it would bring no small accession of strength to the notion here advanced. It would also evince, in conjunction with what has been offered, beyond the possibility of a doubt, that inscriptions of very different kinds have been handed down to us by this species of coins.

That medals should have been emitted from the mint at Tarsus with Phœnician letters upon them, to those versed in this branch of literature can be no matter of surprize. It has been proved, that such pieces as that now in view were (39) struck by the Phœnicians, some of whom were undoubtedly settled at Tarsus, and consequently ought to be deemed Phœnician, not Punic, coins.

As the medal itself, according to the gentleman who purchased it in the East, seems to have been found either in the neighbourhood of Hems, formerly denominated Emefa (40), or somewhere near Esbele, or Gibeles, the Byblus (41) of the antients, and consequently at no very great distance from the borders of Cilicia; my explication of it may possibly, I would flatter myself, prove not altogether unsatisfactory to the learned.

To what has been here advanced some may perhaps object, that we can discover little of the Par-

(38) *Philosoph. Transact.* Vol. LIV. p. 419.

(39) Peller. ubi sup. Tom. Trois. p. 162, 163.

(40) Poc. *Observat. on Syria*, p. 98.

(41) Id. *ibid.*

thian taste or genius in the coins I have been endeavouring to throw some light upon. They may tell us that such pieces seem much more to resemble the medals of Syria and Phœnicia than the Parthian coins. To which I would only beg leave to reply, that I consider them rather as pieces struck at Tarsus by a Greek or Phœnician artist, in some part of the small space of time that the Parthians were masters of Cilicia, than as proper Parthian coins. This appears to me, I own, extremely probable; but that it was an absolute fact, I must not take upon me to affirm.

Thus have I endeavoured to render it probable, from a surprizing concurrence of circumstances, and a wonderful coincidence of facts, that your Lordship's Phœnician coin was struck at Tarsus, the capital of Cilicia, when the Parthians were masters of that country, about forty years before the birth of CHRIST. Whether or no the credibility of such a notion has been here fully evinced, your Lordship, whose profound erudition, as well as singular humanity, is so well known to the whole learned world, with your usual candour and impartiality, will decide. Be this, however, as it will, I shall offer no other apology for any mistakes or omissions that may be discovered in this paper, than that I have been obliged to tread unbeaten paths through the whole course of it; an explication of the medal in question having, as I apprehend, been hitherto unattempted by any considerable writer. I might have expatiated more largely upon the present subject, but am afraid your Lordship will think me already too prolix.

prolix. I shall therefore at this time only beg leave to add, that by honouring me with your future commands, to which the greatest and most ready attention shall ever be paid, you will confer the highest obligation upon,

My Lord,

Your Lordship's very faithful,

and most obedient humble Servant,

Christ-Church, Oxon.  
April 25, 1767.

John Swinton.

ERRATUM, in *Philosoph. Transact.* Vol. LIV, Page \* 139.  
note, l. 2. for *ψψ* read *ΣΨ*, or *Schin, Tzade*.

XXX. *Remarks on the very different Accounts that have been given of the Fecundity of Fishes, with fresh Observations on that Subject: By Mr. Thomas Harmer; communicated by Samuel Clark, Esq; F. R. S.*

Read May 28, 1767. **T**HE ascertaining the fecundity of the several species of fish, as far at least as we are able to do it, is one point necessary to the making our natural histories perfect; and at the same time opens a view wonderfully affecting to the imagination.

The carp, in which Petit is said to have found 342,144 eggs; and the cod, in one of which of middling size Lewenhoeck, it seems, affirmed there were 9,384,000, have been mentioned as very surprizing instances of this fecundity; and by their being selected by writers, who appear to have been well versed in this part of learning, they should seem to be the most memorable we have of this kind.

The accounts, however, that have been given of the fruitfulness of these two species of fish differ from each other very considerably. For Bradley, the Botanic Professor at Cambridge some years ago, tells us in his philosophical account of the works of nature, a book professedly written on a very celebrated, though unexecuted plan,



plan \*, that the increase in some fish is surprizing, and to those that are not used to disquisitions of this kind must appear incredible; he however sinks the number of eggs in these two species extremely, when he tells us the roe of a cod-fish must contain about a million of eggs, and that a carp does not spawn less than 20,000, to which he adds, and perhaps a tench half as many. This is making the cod almost ten times less prolific than the other account, and the carp above seventeen times less. Some other writers, who appear also to have been desirous to impress the mind with the wonders of natural history, have made their estimate still lower. One of them, I remember in particular, in one of our monthly publications, from whence numbers must take their ideas, who have no opportunity of reading more authentic accounts elsewhere, tells us, that carp and perch have nine or ten thousand eggs, and that cod-fish, and herring, are not less prolific; and this he calls wonderful. The increase of cod-fish is indeed, even according to this, very great, but almost a thousand times less than Lewenhoeek is said to have found it.

Their accounts being so very different, I thought I should not improperly employ some leisure hours, if I inquired into this matter afresh, and saw what the fecundity of these species of fish really was, as well as of such other sorts as might fall in my way; and especially as I had found that a small pickerel, whose spawn I had taken a pretty exact account of, from mere curiosity, some time before, contained no fewer than 25,800 eggs: a fish which

\* Proposed by Mr. Addison in one of the Spectators.

none of these authors had mentioned, and of which notwithstanding a small one had produced a larger number of eggs than Bradley himself had assigned to the carp, which has been always looked upon as remarkable for its prolific quality, not to mention the unknown writer, who makes its fecundity much less.

The make of my eyes, which are much less proper for distinguishing objects at a distance, than the seeing small things that are near; and my living in a maritime country, and though not near the sea, yet in a situation which I thought very proper for the procuring such fish as I wanted, were additional motives to the search: though as to the last particular, I have since found myself greatly disappointed.

It will not be imagined, that, in order to ascertain the real number of eggs in each fish which I examined, I told them all over one by one; this would have been, if not absolutely impracticable, at least inconsistent with other engagements, and much more fatiguing than was necessary. My way was to weigh the whole spawn very exactly; then to take a piece weighing twenty, thirty, or forty, or more grains, as was most convenient, and after weighing that parcel with care, and giving the turn of the scales to the weights, not to the eggs, to tell them over very carefully; and then by dividing the number of eggs by the grains, to find how many eggs there were in each grain, or nearly so. I say nearly, for there must, according to this method, have been rather more; but I chose to estimate them after this manner, that there might be no danger of representing the fecundity of these animals greater than the truth

truth. I frequently boiled the portions of spawn that I told, and after macerating them some hours in water, gently pressed them with a penknife, whose point I afterwards made use of to number them distinctly, by separating them from each other to greater distances, after they were rendered by that gentle squeezing fitter for telling over.

In several fish I found their eggs of very different sizes. In such cases my rule was, to tell all I could distinguish by my naked eye, and those only; though I have often found, by the help of an eye-glass, that, by thus limiting myself, I passed over great multitudes of eggs that might justly have been counted\*. I generally told them on a fine earthen

\* For, though they were very different in size, they were all, I presume, to be deposited in the proper places for hatching that season, though it may be not on the same day, since after fish are shotten, as it is called, we find no eggs at all in them. I think I do not by any means take upon me to affirm, that all fish deposite their eggs after this manner, i. e. by degrees, and at times a little distant from each other; but some species of them, I should think it is plain, do. This is the case of stickleback in particular, in which, when they have been extremely distended with spawn, I have found several eggs very large, but several hundreds very small, and many of them too small to be counted distinctly by an unassisted eye, which smaller ones could never, I apprehend, grow to the size the larger ones had grown to before the larger ones were excluded. I have seen some of these eggs so large, that 24 or 25 would weigh a grain; about which size, I believe, they are excluded, since some of that bulk came away from one of those creatures after it was taken out of the water, and were found in the paper in which it was wrapped up: but generally upon opening these fish they are not quite so large, though very large in comparison of many of the rest. I told in one of these smallest of fishes, which weighed very little more than 14 gr. spawn and all, about 936 eggs, some of which almost eluded my eye, besides

vessel, which was extremely black, by which means I could much better discern them, than I otherwise should have been able to do. The weights I have reckoned by are Avoirdupois weights; but there being no weights of that sort small enough to answer all purposes, I was obliged to make use of grains along with them, of which I reckon  $437\frac{1}{2}$  make an ounce Avoirdupois. After this manner I made the observations of which I am going to give an account, with all the nicety and care I was capable of.

I begin with the herring, which makes a distinguished figure in these two counties of Norfolk and Suffolk, and a considerable part of our commerce, when salted and smoked. One of the above-mentioned authors supposed, they may have 9 or 10,000 eggs: of several I examined, I found none which had so few as 20,000; and in one I found 36,960. The intermediate numbers which appeared to me on

numbers of others that I could not tell at all. Those that I told were of all sizes; and though there were but 56 very large eggs, yet the creature was extremely distended. Now it does not appear possible that all the 936 eggs should grow to the bulk of the largest of their companions without destroying the fish, since the growing of 56 to such a size as to weigh about  $1\frac{1}{2}$  grain distended it in such a manner as I have hardly seen in any other species of fish. In some very large sticklebacks, which weighed 45 or 50 grains, I have found between 2 and 300 of these large eggs, along with great numbers of smaller ones, so that the number of large eggs seems to bear some proportion to the size of the fish, by which means a greater number of eggs, answerable to their size, may be deposited in the same time in which the smaller fish of this species discharge their less numerous spawn. I will only farther observe here, that if this be the case as to the other fish, the number of their eggs may, on this account, appear very different to different observers.

examination,

examination, may be met with in a table that I shall place at the end of this paper, which will give the particulars relating to this sort of fish, and several others, in the shortest manner, but with sufficient distinctness.

The next that came under my inspection was the smelt. These, it is well known, are a very small sort of fish, and are frequently used for garnish to those that are larger. In one of these, which did not weigh quite two ounces, I found 38,272 eggs; and in none so few as 20,000; excepting one, which was extremely small, not weighing above  $289\frac{1}{2}$  grains, in which very small fish I found 14,411. This was amazing.

I was much more surprized, when, after this, I learnt what was the fecundity of mackarel. This no author that I met with gave an account of, though it is a fish so extremely common. In one large fish of this kind, weighing somewhat better than 1 $\frac{1}{4}$  lb, I found 454,961 eggs; in a second, of much the same weight, 430,846; and in a third, which weighed but about 1 lb 2 oz. I found 546,681.

I was astonished upon this, that Bradley should call the supposed fecundity of carp, which he makes to be but about 20,000, so surprizing; or that even Petit's observation, which made it appear, that in some fish of that species the eggs amount to 342,144, should cause the carp to be selected as the most extraordinary fish for increase, after the cod, when it appears to be so much greater in mackarel (which is at the same time so common a fish), as to be not much short of the proportion of 5 to 3, in the last I examined.

I shall

I shall leave it to the table at the close, to give an account of the prolific quality of some other fish, whose eggs I have counted, and shall pass on to what I have found in the carp, which species Petit examined. As to this fish, though I cannot say I have found the eggs, in those that have come under my notice, so numerous as he did; yet, as I have found the number much larger than Bradley mentions, so I make no doubt but that Petit really found them amount to 342,144; and I would add, that I dare say they may be found to be much more numerous still in large fish of this kind, since in one that I examined, weighing but  $16\frac{3}{4}$  oz. I found 101,200; and in another, which weighed no more than  $25\frac{1}{2}$  oz. I found 203,109, and carp grow to a much larger size than the biggest of these, but I could not procure any of those large carp that were full of spawn.

If I failed in verifying Petit's number of eggs in a carp, I found those of a tench to exceed it, more than once, which Bradley reckons not to produce more than half the number a carp does, or 10,000. For I found in one tench, which weighed  $2\frac{1}{2}$  lb, 383,252 eggs at least; and in another that weighed not quite  $1\frac{1}{2}$  lb, 350,482.

As to perch, which one of those authors I mentioned puts upon a par with carp, I could get none of any size. The largest, which weighed but 8 oz. 9 dr. contained 28,323 eggs; and a second of 5 oz. 10 dr. had in it 20,582. They seem, however, to be much less prolific, in proportion to their bulk, than tench, since the largest of these had but about 28,323 eggs, and I found a tench of nearly that size, weighing but 8 oz.  $14\frac{3}{4}$  dr. produced 83,104.

Bradley

Bradley seems to have been as careful not to be guilty of exaggeration with respect to the cod-fish, as in the other cases. He estimates them at a million, while Lewenhoeck affirmed that he found above nine millions of eggs, in one of middling size. The spawn of one of that size which I examined, that is to say, one of 18 or 20 lb weight, I found to contain between three and four millions, if my friend's weights were accurate, for I made the examination at a distance from home; and by a subsequent observation at home, I have reason to think there was this number in it, though I was prevented pursuing my second examination so far as I would have done, by some particular occurrences. According to this, there is nothing incredible in Lewenhoeck's account; his fish, I suppose, being remarkably distended with spawn, and for that reason perhaps thought, by that inquisitive and curious person, a subject that ought not to pass unexamined.

I have hitherto mentioned no flat fish, nor do I remember that any author has given us an account of their fecundity. I imagined, from their make, it could not be extraordinary; I was therefore extremely surprized to find in the first flounder I examined and which did not weigh quite three ounces, 133,407 eggs; in a second, which weighed little more than  $3\frac{1}{2}$  ounces, 225,568; and much more still when I discovered in a large one, that weighed about  $24\frac{1}{2}$  oz, and which was of that sort that is spotted like a plaice, as Ray has told us some flounders are, 1,357,400: this was truly astonishing.

The number of eggs that a shoal produces, I have observed to be great, but nothing like that which I

found in flounders, finding in one, which did not fall much short of a pound, rather more than 100,000; and in another, that weighed about five ounces, 38,772.

To make this disquisition still more extensive, I examined two or three kinds of shell-fish. I found in a lobster of  $14\frac{1}{2}$  oz. when in the shell, and of  $10\frac{1}{4}$ , when divested of it, 7,227; and in another that weighed  $2\frac{1}{4}$  lb, and out of the shell somewhat better than  $1\frac{3}{4}$  lb, I told 21,699 eggs.

I took also the pains to tell all the eggs of some shrimps, one by one, and found in one which weighed  $17\frac{1}{2}$  grains only, 3057; in another of 31 gr. 4090; and in a third of 39 gr. 6807.

This, considering the smallness of the creature, is more remarkable than the fruitfulness of the lobster; but neither is the one or the other to be compared, in this respect, to the crab, for in a large one, weighing near  $1\frac{1}{2}$  lb, but not quite, I found that the spawn weighed 687 gr. but the eggs were so minute, and at the same time adhered so close together, that I could not number them distinctly; however, the weight of the whole, and the minuteness of the eggs, show that they must have been very numerous; and I believe from a rough estimate I made, that they exceeded a million.

The table, at the close, gives all these accounts, with the addition of several more particulars, in a very short compass. The first column contains the names of the fish which I examined; the second their weight; the third the weight of their spawn; the fourth their fecundity; and, as I supposed some persons might be desirous to know how large a portion



portion of spawn I weighed in each case, I have set down the number of grains in each such portion, in a fifth; the number of eggs found in a grain, by this method is seen in a sixth, by which we may give some guess at the different sizes the eggs of each species are of, when they are excluded; and I have made the time of examining each fish respectively a seventh; which possibly may be of some use to those who may have an inclination to repeat any of these observations, as from thence may be learnt something concerning the most advantageous time of examining these creatures, which certainly ought to be as nearly as we can, when the eggs are come to their full size, and before any of them are deposited. However, after all, if my notion is just, that some species deposit a part of their eggs come to their full growth, before others laid the same year are big enough to be told with distinctness, the accounts of the fecundity of such fish must be extremely defective; and this I apprehend, amongst those I have examined, is the case of mackarel, carp, tench, and some others; in herring, &c. there does not appear such a difference in the size of different eggs.

From this table it appears, that the size of the eggs is nearly the same in great and small fishes of the same species, at the same time of the year; that the quantity of spawn is, usually, nearly proportionate to the size of the animal, from whence we may give a tolerable guess at the greatest fecundity of each species, if we know to what weight they have been found to grow while in a breeding state; we may likewise settle their produce at a medium, upon learning what is the mean size of each species.

when in the forementioned state, but we see, however, that this is not universal, nor consequently perfectly exact, some fish being much more prolific than others of the same size, and species.

To conclude, the great fecundity of fish is not the only thing that affects the imagination, when we are examining matters of this sort: the extreme disproportion between their size when they first appear in the water after hatching, and that of their full-grown state, as well as the little proportion that is to be observed between the bulk of fish of different species and that of their eggs, are things that are very amazing to persons of a curious turn. The egg of a smelt, which at its full growth weighs but two or three ounces, appeared, in those I examined, to be larger than those of a cod-fish, which weighed eighteen or twenty pounds, and might have grown to double that bulk; and that of a stickleback, which is the smallest of all known fish, was found to be above six times bigger than the largest I ever observed in a smelt. What becomes of such amazing numbers of young fish, and why some are made so extremely prolific, the flounder and crab in particular among the smaller sorts, would doubtless be highly entertaining subjects, if duly illustrated; but these are enquiries I have no opportunity of making.

The TABLE.

1. Names of the fish.		2. Their weight	3. Weight of the spawn	4. Fecundity	5. The portion of sp. weighed	6. N <sup>o</sup> . of eggs to a grain	7. Time of exam.
		oz. dr.	grains.		grains.		
Carp	N <sup>o</sup> . 1	16 12	1265	101.200	46	80	May 25
	N <sup>o</sup> . 2	25 8	2571	203.109	55	79	April 4
Cod-fish		—	12.540	3.686.760	29	294	Dec. 23
Flounder	N <sup>o</sup> . 1	2 14	182½	133.407	23	73½	Feb. 21
	N <sup>o</sup> . 2	3 8½	152	225.568	19	1484	Dec. 18
	N <sup>o</sup> . 3	6 12	598	351.026	26½	587	March 14
	N <sup>o</sup> . 4	24 4	2.200	1.357.400	24½	617	d <sup>o</sup> .
Herring	N <sup>o</sup> . 1	4 3	367	32.663	48	89	Oct. 8, 1763
	N <sup>o</sup> . 2	5	236½	21.285	48½	90	29
	N <sup>o</sup> . 3	3 13	259	23.569	52½	91	Oct. 2, 1764
	N <sup>o</sup> . 4	5 10	480	36.960	53	77	25
	N <sup>o</sup> . 5	4 6½	366	29.646	57	81	d <sup>o</sup> .
	N <sup>o</sup> . 6	4 8	420½	27.753	51	66	Nov. 3
	N <sup>o</sup> . 7	5 1	490½	32.863	41½	67	Oct. 18
Lobster	N <sup>o</sup> . 1	14 8	—	7227	—	14	April 4
	N <sup>o</sup> . 2	36 0	1671	21.699	129	—	Aug. 11
Mackarel	N <sup>o</sup> . 1	20 —	1027	454.961	33	443	June 20, 1764
	N <sup>o</sup> . 2	20 —	949	430.846	24½	454	29
	N <sup>o</sup> . 3	18 —	1223½	546.681	32½	447	18, 1765
Perch	N <sup>o</sup> . 1	8 9	765½	28.323	85	37	April 5
	N <sup>o</sup> . 2	5 10	502	20.582	85	41	6
Pickerel	N <sup>o</sup> . 1	56 4	5100½	49.304	70	9½	April 25
	N <sup>o</sup> . 2	—	3248	80.388	76½	24½	Nov. 25
	N <sup>o</sup> . 3	48 10½	3184	33.432	43	10½	March 19
Prawn	N <sup>o</sup> . 1	(127 gr.)	—	3.806	—	243	May 12
	N <sup>o</sup> . 2	(94½ gr.)	—	3.479	—	287	d <sup>o</sup> .
	N <sup>o</sup> . 3	(100½ gr.)	—	3.579	—	247	d <sup>o</sup> .

Roach

1. Names of the fish.	2. Their weight	3. Weight of the spawn	4. Fecundity	5. The portion of sp. weighed	6. N <sup>o</sup> . of eggs to a grain	7. Time of exam.
	oz. dr.	grains.		grains.		
Roach (or what I took to be of that species)	N <sup>o</sup> . 1	2 —	114	9.604	—	April 4
	N <sup>o</sup> . 2	6 8	671	43.615	68	May 4 1764
	N <sup>o</sup> . 3	3 8	346½	29.799	42½	d <sup>o</sup> .
	N <sup>o</sup> . 4	2 2	153	9.486	42½	5
	N <sup>o</sup> . 5	10 6½	361	81.586	39	2 1765
	N <sup>o</sup> . 6	9 10½	417	113.841	42	6
	N <sup>o</sup> . 7	3 8	213½	45.475	20	24
Shrimp (with light coloured spawn.)	N <sup>o</sup> . 1	(17½ gr.)	3	3.057	1000	May 3
	N <sup>o</sup> . 2	(39 gr.)	7	6.807	972	d <sup>o</sup> .
	N <sup>o</sup> . 3	—	—	4.601	—	d <sup>o</sup> .
Ditto (with dark colour)	N <sup>o</sup> . 1	(31 gr.)	5	4.090	818	d <sup>o</sup> .
	N <sup>o</sup> . 2	(22 gr.)	4	2.849	712	d <sup>o</sup> .
Smelt	N <sup>o</sup> . 1	2 —	149½	38.278	30	Feb. 21
	N <sup>o</sup> . 2	(289½ gr.)	50	14.411	—	Mar. 21, 1764
	N <sup>o</sup> . 3	1 14	157½	29.925	40½	27, 1765
	N <sup>o</sup> . 4	1 12	145½	30.991	20	28
	N <sup>o</sup> . 5	1 7	149	24.287	20	d <sup>o</sup> .
	N <sup>o</sup> . 6	1 5	136	23.800	20	d <sup>o</sup> .
Soal	N <sup>o</sup> . 1	14 8	542½	100.362	20	June 13
	N <sup>o</sup> . 2	5 —	179½	38.772	20	28
Tench	N <sup>o</sup> . 1	40 —	—	383.252*	—	May 28, 1764
	N <sup>o</sup> . 2	28 8	533½	280.087	25	3, 1765
	N <sup>o</sup> . 3	8 14½	224	83.104	20	10
	N <sup>o</sup> . 4	9 8	284½	108.963	20	d <sup>o</sup> .
	N <sup>o</sup> . 5	12 8	366	138.348	22½	d <sup>o</sup> .
	N <sup>o</sup> . 6	27 9½	1969	350.482	23	June 11
	N <sup>o</sup> . 7	14 15*	866	138.560	20	d <sup>o</sup> .

\* N<sup>o</sup>. 1. of the tench certainly had a much larger number of eggs; but being extremely distended with spawn, and unluckily let fall before it was brought to me, the enveloping skin in which the eggs were contained was broke, which made it difficult to determine some circumstances relating to this fish; it however had the number of eggs I have set down, at the lowest way of reckoning, and I believe many thousands more.

I have taken no notice of several fractions in the number of eggs contained in a grain in many cases, choosing to fall rather below than to exceed the truth, in all the fish I have given an account of in this table. I have been scrupulously exact in all particulars, excepting what are contained in the second column, which gives the weight of the fish I examined, in which the utmost nicety was not necessary: some few might weigh a little more or a little less; but all were nearly of the weight set down, and much the greatest part exactly so.

XXXI. *An Account of an Hydro-enterocele, appearing like an Hydro-sarcocele, and ending in the Death of the Patient, in which the Intestine had passed from the Hernial Sac, into that of the Hydrocele by which the Strangulation was formed. Communicated by Mr. Le Cat, F. R. S. &c. in a Letter to Charles Morton, Sec. R. S. Translated by J. O. Justamond, Surgeon.*

Read May 28, 1767. **I**N the year 1751, I presented to the Royal Society an observation of a rupture, which had two hernial sacs; at present, I have the honour to send them the history of another kind of rupture with a double sac, much more fallacious and dangerous than the former, since I found myself deceived by it, after the experience of forty years.

James Philip Chiquet, aged 65 years, of the Parish of St. Vivien at Rouen, was admitted into our hospital on the 24th of January 1767.

The account, which he gave of himself, was, that he had been accustomed to a rupture, which he had not been able to reduce for a fortnight past, and, that since eight days he had been seized with a vomiting, and was incapable of taking any nourishment.

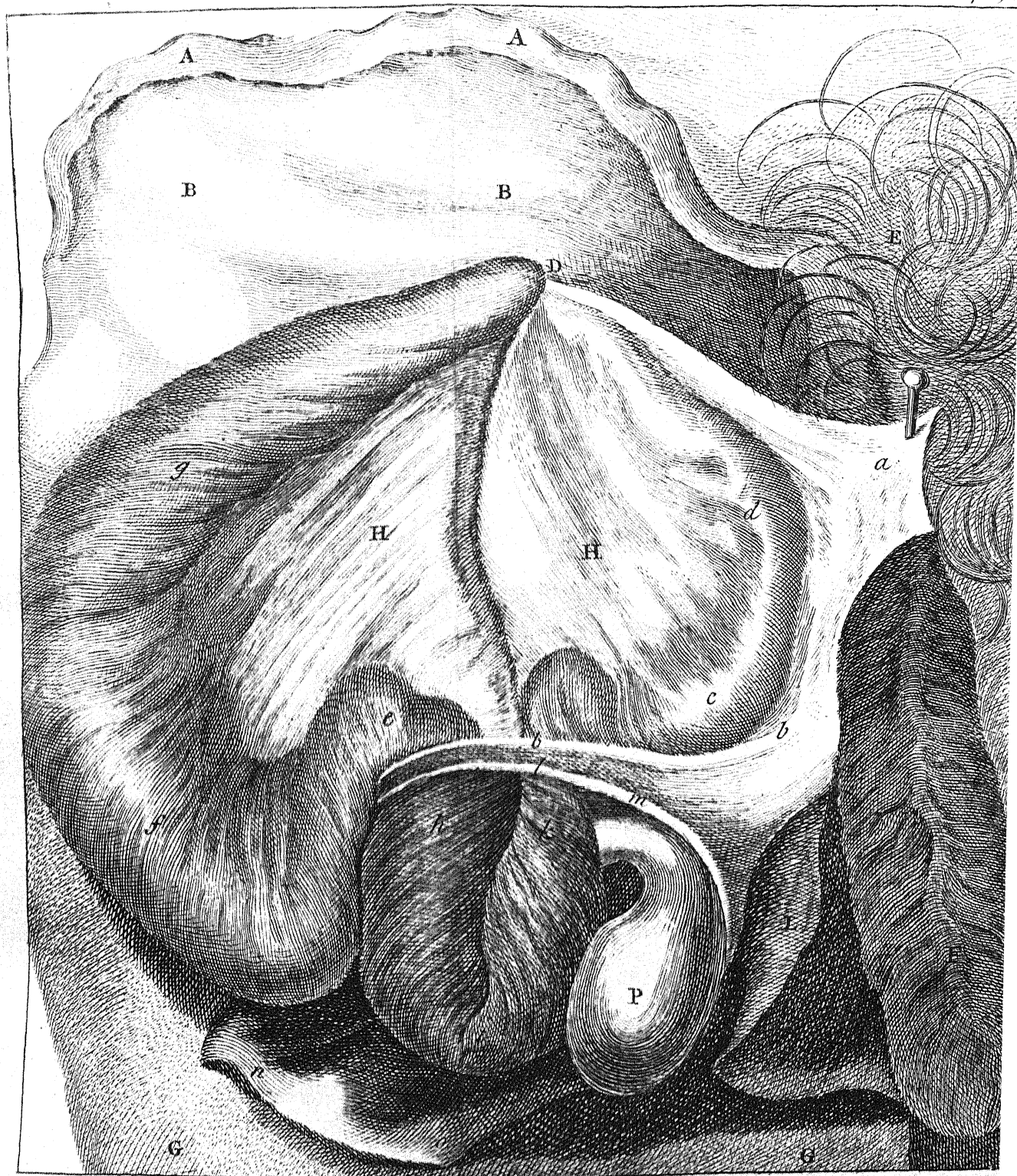
ment. Upon examination, the tumor was soft, especially at the upper part towards the ring, which seemed to be so fine and disengaged, that the finger with the integuments might be pushed under it: the large cord, which came down to it, was flat, soft, and appeared to be composed entirely of the spermatic vessels enlarged.

The extremity of the swelling, which was of the size of a large orange, was evidently a very transparent hydrocele; at the basis of which some hard points were to be felt, which I thought to be scirrhus tumors. I concluded, therefore, that his complaint was an old hernia, succeeded by a sarcocele and an hydrocele, and that the intestine was at that time returned. I imagined that the vomitings, which were not frequent, might be caused by some other disorder, perhaps by the progress which the sarcocele might have made in the cavity of the abdomen; and I also thought that the weak and almost dying state he appeared to be in, was a prognostic of the fatal manner in which those cases usually terminate; for his strength was so far exhausted, that he expired in the following night.

I was very desirous of examining the case, having been always induced to suspect, from the vomitings, and the flatness, softness, and size of what passed through the ring, that there was a descent of the intestine.

See TAB. XIII. Upon opening the common hernial sac, *a. b. b.* a large portion of intestine, *c. d. e. f. g.* presented itself, which was very flaccid and almost empty; but what surprized me most, was, to find that the convoluted











voluted extremity, *b. i. k.* of this intestine, had insinuated itself into the sac, *l. m. n. o.* of the hydrocele, which was formed by the vaginal coat of the testicle, *P.* and, that only this portion of intestine, *b. i. k.* was strangulated, hard, and changed in colour. The redness was so slight, that this strangulation could scarcely have been the immediate cause of his death; but I rather imagined that an universal decay, and waste of strength, for a long time past, had contributed to hasten this event.

If the patient had lived, my design was, first, to have drawn off the water of the hydrocele by an opening made large enough to have examined into the contents of the tumor, which were but indistinctly felt, on account of the surrounding water.

According to the discovery I should have made by this opening, I should then have pursued my operation, whether I had met with a sarcocele or a rupture. In order to put these intentions into execution, I had previously ordered the hair to be shaved off the tumor, and, on account of his great weakness, forbade bleeding, which had been ordered at first. This operation would probably have saved his life, if he had come into the hospital a few days sooner, and his strength had been sufficient to carry him through it.

### Explanation of Plate XIII.

A. The integuments of the abdomen raised. B. The external oblique muscle. D. The ring. E. The Pubis. F. The Penis. G. The right thigh. H. The mesentery. L. Part of the dartos.

*a. b. b.* Part of the hernial sac.

*c. d. e. f. g.* Portion of intestine contained in this sac.

*h. i. k.* The convoluted extremity of this intestine contained in the sac of the hydrocele.

*l. m. n. o.* The sac of the hydrocele composed of the vaginal coat of the testicle.

*P.* The testicle.

XXXII. *Novorum quorundam in re electrica experimentorum Specimen, quod Regiæ Londinensi Societati mittebat die 26 Aprilis 1766, Joannes Baptista Beccaria, ex Scholis Piis, R. S. Soc. communicated by M. Maty, Sec. R. S.*

EXPERIMENTUM PRIMUM.

- Read June 4, 1767. **E**XPERIMENTUM captum Pekini a PP. Soc. JESU, & anno 1755, missum academici Petropolitane, <sup>a</sup> analysi persequi studens, laminam vitream lævem A charta inaurata rite indutam faciebam electricam, immisso in ipsam igne a catena; tum exutam imam ipsius faciem admovebam faciei superiori laminæ similis B utrinque nudæ, nec ullatenus electricæ, quæ facie sua ima circello imminebat chartaceo sesquipollicari <sup>b</sup>.
- II. Porro, nisi a facie superiore vitri A ignem redundantem haurirem aliquomodo, circellus non commovebatur; ubi attingebam ejus faciei indusium, circellus emicabat ex abaco, oscillaba-

<sup>a</sup> Vide Nov. commentar. Acad. Scien. Imp. Petrop. tom. viii. pag. 276.

<sup>b</sup> Lamina A pendebat uncias 6. octavas uncie partes 7; lamina B pendebat uncias 6. octavas uncie partes 4. denarium 1; utriusque laminæ latitudo erat pollicum 6. linearum 10; longitudo pollicum 8. linearum 11; latitudo indusiorum pollicum 4; longitudo pollicum 5.

tur; imo erectus circuibat quasi saliens sub ea lamina; quales aptissimas positiones, & motus facit sibi in traducentibus corporibus trajiciens electricus ignis. Ii motus, dum divellebam laminam A, abrumpebantur; ea iterum admota, reviviscebant motus eorum omnium residui, quos facere debuisset attréctatio indusii laminæ A, nisi fuisset ipsa dimota; attréctatio alia similem, sed debiliorem motuum, seriem excitabat, atque ita concidebat vis explodendi, & quatiendi in lamina A.

III. Quum primo disjungerem laminam A a lamina B, jam percipiebam, insolita vi aliqua cohærere mutuo ambas; eamque cohæsiōnem experiebar majorem, prout tardius disjungebam laminam A, post plures scilicet attréctationes. Si, cum primo laminam A imposueram laminæ B, induebam extimam hujus faciem, attréctando simul indusia junctorum vitrorum, quatiebar: valida repente existerat ab ea explosione vitrorum cohæsiō, eademque validior pro explosione vehementiore.

IV. Quum laminas ante explosiōem jam aliquomodo cohærentes disjungerem, lamina A utraque facie apparebat electrica excessu, & lamina B utraque facie apparebat electrica defectu; quum laminas easdem disjungere post explosiōem, electricitates apparebant contrariæ; scilicet lamina A apparebat utrinque electrica defectu, lamina B utrinque excessu. *Quam mutationem primam nomino ex explosiōe electricitatum oscillationem.*

V. Si post explosiōem alternatim disjungebam, & conjungebam laminas prope circellum chartaceum (I.), circellus in quaque disjunctiōe adhærebat laminæ

laminæ B, in quaque conjunctione discedebat, idque vel quingenties; quod est experimentum Pekinense, quodque innuit *alias, quæ fiunt ex disjunctione et conjunctione vitrorum, electricitatum oscillationes.*

VI. Has scintillis facile imitabar: vitrum A electricum a catena (I.) exutum ima facie imponebam nudæ faciei vitri B; indutam faciem extimam hujus vitri, & indusium vitri A una attrectabam; quatiebar: explosionem consummabam iterata attrectatione, aut aliter; tum vitra juncta juxta latus unum meo apprimebam stomacho summis ipsa prehensens juxta latus oppositum manus utriusque digitis, quo disjungere alternatim ipsa possem, atque iterum conjungere. Porro manus habebam ita dispositas, ut auriculari digito manus lævæ indusium laminæ B possem attingere, & pollice manus dexteræ possem similiter attingere indusium laminæ A. Hæc tunc observabam.

VII. 1. Si ante, & post disjunctionem vitrorum indusia eorum ambo attrectarem, scintillas percipiebam utrinque; sed alacriores, quæ existebant a disjunctione: at istæ erant ferme unicæ. Quæ autem conjunctionem sequebantur, erant multiplices, ut viderentur multiplicitate sua illarum vim exæquare; atque si eadem lege pergebam vel octingenties, uti sum semel expertus, vitra divellere, atque conjungere, scintillæ nondum omnino peribant. 2. Si disjunctis vitris, neutrum attrectabam, post eorundem conjunctionem scintillas nullas percipiebam, percipere poteram in sequente disjunctione; similiter si conjunctis vitris, neutrum attrectarem, post disjunctionem scintillas non percipiebam,

cipiebam, percipiebam post sequentem conjunctionem. 3. Si post disjunctionem attrectarem vitrorum unum dumtaxat, aut nullæ, aut omnino exiguæ post conjunctionem scintillæ percipiebantur; similiter, si post conjunctionem attingebam unicum vitrum, scintillæ post disjunctionem aut sentiebantur nullæ, aut vix sentiebantur aliquæ.

VIII. Re ita scintillis expensa, penicillo, & stellula earum scintillarum directionem persequer: facies superior vitri A, quod unum evaserat electricum a catena, hisce in divulsionibus post explosionem penicillum, facies extima vitri B stellulam efformabat in objectis apicibus; & exploratore immisso inter vitra divulsa (filo nempe ferreo abeunte in ramos duos) facies intima vitri A penicillum, intima vitri B stellulam efformabat; scilicet vitrum A a divulsione post explosionem erat utrinque electricum defectu, vitrum B. utrinque excessu.

IX. Contra in conjunctione facies extima vitri A efformabat stellulam, facies extima vitri B in exhibitis apicibus penicillum faciebat; quæ duæ res (VIII. IX.) veram demonstrant, continuatamque electricitatum ex disjunctione, & conjunctione oscillationem.

X. Ipso conjunctionis, & disjunctionis momento facies internæ quid facerent, nequivi cernere; sed ex hac experimentorum serie poterit quisque arguere.

XI. Atque hæc quidem altera ex vitrorum conjunctione, & disjunctione oscillatio jucunda est admodum; sed & aliam præterea licet cum ipsa componere, quæ efficitur ex inversione vitri præpol-



lentis; in hoc enim experimento vitrum illud, quod principio factum est electricum a catena, quum facie contraria tangit vitrum aliud, contrarias & in ipso se, & in vitro socio electricitates facit, tum quæ sunt ex disjunctione, tum quæ conjunctionem sequuntur; seu planius: si inter hæc experimenta invertitur vitrum A, ut facies, quæ erat extima, denudata evadat intima, & facies, quæ erat intima, fiat extima, & induatur, nova aliqua post aliquod tempus ex apta attractione oppositarum facierum insurgit vitrorum co-hæsiō; & vitrum A, quod a disjunctione apparebat utrinque electricum defectu, nunc apparet utrinque electricum excessu; vitrum B, quod a disjunctione apparebat utrinque electricum excessu, nunc apparet utrinque electricum defectu; & in conjunctione vitrum A, quod apparebat electricum excessu, nunc apparet electricum defectu; vitrum B, quod in conjunctione apparebat electricum defectu, nunc apparet electricum excessu. Atque tanta est præpollentis vitri A vis, ut, siue interea vitrum B non invertatur, siue ipsum etiam invertatur, descripta electricitatum mutatio semper contingat, nulla unquam contingat, si solum vitrum B invertatur. Præterea si vitrum A restituatur suo loco, ut facies ipsius, quæ principio fuerat intima, & ab inversione evaserat extima, iterum denudetur, & fiat intima, iterum redeunt eadem, quæ principio fuerant electricitates, iterum mutandæ ab alia inversione vitri A, iterum restituendæ ab ejus vitri restitutione in positionem primam. Atque ita deinceps.

XII. Porro vitrorum inversiones contrarias electricitates consequentes ex vitrorum conjunctione, et disjunctione, debilitant longe magis, quam debilitent solæ disjunctiones, conjunctionesque nulla interposita vitrorum inversione; potui tamen cælo mediocriter sicco quinquies & decies memoratas electricitatum ex inversione vitri A mutationes observare, & interponere præterea inversiones plures vitri B, quo viderem, his electricitates non mutari.

XIII. Quas tamen observationum progressionem ut assequi possimus, primum, ut innuebam, tempestas facit, tum vero mutationibus electricitatum pervidendis opportuna vitrorum junctorum post inversionem mora, & attrectatio est plane necessaria; mora autem sufficiet brevior, prout vitrorum junctorum facies extimæ corporibus attrectabuntur ad ignem electricum hauriendum, immittendumque aptioribus. Ego quandoque, cum citius vitra disjungerem, aut electricitates nondum mutatas in contrarias, aut contrarias nondum satis invaluisse, videbam, & faciebam, ut invalescerent, vitra iterum jungendo faciebus iisdem, apteque attrectando.

XIV. Ad reliqua quod attinet experimenti adjuncta ea sunt pleraque omnino obvia. Unum memorabo, quod & ipsum, quamquam se prodat satis aperte, meretur tamen recenseri ideo, quod singularem aliquam habeat significandi vim. Itaque cum primo ab opportuna post explosionem mora vitra disjunguntur, tum scintillæ existunt inter angulos induriorum, & digitos, quibus nudos vitrorum angulos prehendo, omnino vividæ; intereaque scintillarum crepitus exauditur undique plurimus,

plurimus, atque, si res agatur in tenebris, lux plurima circa margines indusiorum, & plurima emicat intra intimas vitrorum facies: tanta nempe est vis, qua a disjunctione turbatur æquilibrium, quod jam ferme obtinuerat ante disjunctionem. Neque hæc phænomena in ea prima disjunctione finiuntur, sed, quamquam usque & usque debiliora, apparere tamen pergunt in disjunctionibus aliis, & si cætera sint paria, alacriora obveniunt, prout rapidiore motu distrahuntur vitra, & prout a debita mora, & attrectatione invaluerunt magis novæ pro nova disjunctione electricitates. Sive autem experirer faciebus vitrorum indutis, sive nudatis faciebus vitra conjungerem, atque disjungerem, phænomena semper existebant homologa.

#### EXPERIMENTUM SECUNDUM.

XV. Hæc experimenti Pekinenfis analysi deducebat me ad experimentum Simmerianum explorandum & promovendum. Utebar iisdem duobus vitris A & B, quæ juncta faciebus nudis induebam faciebus duabus aliis extimis, & aptabam catenæ, ut fierent electrica<sup>c</sup>. Intereaque jucundum erat mihi oculis cernere invalescentem in ipsis electricitatem; etenim circa indusia zonæ coloratæ Newtonianæ conformabantur, quas commode cernebam in distantia pedum quatuor, & distinguebam numero sex. Eæ parallelæ erant omnes inter se, & erant pariter ferme parallelæ indusiorum limitibus. Ferme, inquam, nam circa indu-

<sup>c</sup> Vitra semper adhibui prota tersa & sicca ab igne.

fiorum angulos, quos, ne electricitatem perderent, rotundaveram, cæ zonæ arcuabantur in semicirculos ab indusiis extrorsum prominentes magis, quam zonæ parallelæ lateribus; quamquam hæ etiam sinuabantur introrsum ubi indusiorum latera vitris non adhærebant. Sed de his alias plura, nec, ut arbitror, inutilia; sunt enim hæ zonæ vestigium conspicuum electricitatis se effundentis circa limites rectilineos, arcuatos, acutos, &c.

XVI. Vitra quum cernebam omnino electrica, dimovebam a catena, attrectabam in extimis indusiis, fiebat explosio, quatiebar, neque interea ab explosione mutari animadvertēbam coloratas zonas indicia cohæsionis.

XVII. Re enim vera obniti debebam valide admodum, ut vitra memorata disjungerem; quam primum vero hanc disjunctionem me assequi sentiebam, evanescebant zonæ coloratæ; intereaque scintillæ, crepitus, lux, & phænomena existebant omnia, quæ memoravi (XIV), imo iis alacriora.

XVIII. Porro si vitra disjungerem anteaquam attrectando extrema indusia explosionem cicerem, vitrum superius apparebat utrinque electricum excessu, inferius utrinque defectu: Si deinde explosionem conficerem, a disjunctione, quæ postea fiebat, mutatæ apparebant electricitates; vitrum superius erat utrinque electricum defectu, inferius excessu.

XIX. Si vitra post explosionem disungere, & conjungere pergebam, quemadmodum in superiore experimento, eadem, quæ in illo, imo vividiores consequēbantur electricitatum oscillationes, quas similiter penicillo expēdebam, & stellula.

XX. Neque

XX. Neque hic deerat vitrum præpollens, quod aliam componeret ex sua inversione cum ea oscillatione electricitatum oscillationem. Hæc una erat, ipsaque aptissima differentia: Vitrum præpollens in superiore experimento illud erat, quod unum fiebat electricum a catena. At in hoc experimento, in quo ambo junctim fiebant electrica, præpollebat vitrum tenuius<sup>d</sup>, sive ipsum catenam tangeret, dum fiebant electrica a catena, sive esset inferius, & communicaret cum solo.

XXI. Præpollens vero vitrum hoc suam vim primo demonstrabat in explosione, quæ tentaretur in duobus vitris seorsim: scilicet si post explosionem junctorum vitrorum intimis faciebus disjuntorum aptabam indusia, vitrum præpollens me quatiebat mediocriter, aliud non item.

XXII. Itaque si, quemadmodum in superiore experimento, ita hic post explosionem invertebatur vitrum præpollens, electricitates a disjunctione, et conjunctione existentes mutabantur similiter in contrarias, restituendæ restituto vitro præpollente, iterum mutandæ eo iterum inverso, neque mutandæ inversione vitri alterius sola.

XXIII. Atque hanc hujus suæ vis tenacitatem pergebam adhuc aliter experiri in vitro præpollente. Duo vitra junctim electrica a catena statim divellebam, qua prima in disjunctione scintillæ, lux, crepitus, phænomena obtingebant omnia iis si-

<sup>d</sup> Plura in his experimentis adhibui vitrorum paria, & semper tum in hoc, tum in experimentis, quæ sequuntur, illud præpollebat, quod erat tenuius; nunc in par vitrorum incidi, quorum alterum, quod præpolleret, crassius videtur, sed videtur etiam densitate minore.

milia, quæ memoravi (XIV); tum induebam intimas eorum facies, vitrum præpollens attrectabam, quætiebar; consummata explosione ipsum aptissime attrectabam (XIII), post minutum temporis aptabam vitro socio, quod interea pependerat in aëre summo angulo suspensum; juncta attrectabam, vix existerat explosio; disjuncta electricitates monstrabant iis contrarias, quas solent ostendere ante explosionem. Atque tum a disjunctiōe, & conjunctione consueta post explosionem electricitates oscillabantur; istæque invertiebantur inverso vitro præpollente.

XXIV. Vitra eadem junctim electrica tentabam, ut exploderent juncta, deinde sejuncta, iterum juncta, iterum sejuncta; postea attrectabam dudum seorsim singula (XIII) ad minutum temporis, iterum juncta eadem, qua principio fuerant, positione, post debitam moram, & attrectationem, electricitates monstrabant singula contrarias electricitatibus ante explosionem; & tum etiam ab inversione vitri præpollentis eæ electricitates abibant in contrarias.

### EXPERIMENTUM TERTIUM.

XXV. Duo vitra A & B singula rite induta singulis catenæ ramis objiciebam, ut fierent seorsim electrica; atque, ut æquilibratas electricitates servarent, quales nempe eodem tempore a catena eadem immitti potuerant, ea cautione a catena ipsa dimovebam, ut neuter ejus ramus cum solo communicaret nisi post remota vitra ambo.

XVI. Mox

**XVI.** Mox denudatis duabus imis vitrorum faciebus, (quod dum fiebat, scintillulæ existerant singulæ ex singulis imis vitrorum faciebus) aptabam vitrum alterum alteri, ut faciebus denudatis se mutuo contingerent; atque tum nec ex attrectatione extimorum indusiorum ulla existerat explosio, nec ullam percipiebam cohæsiõnem in disjunctiõne: sejuncta, atque iterum induta quatiebant valide admodum, sed tenuius validius; iterum juncta faciebus iisdem denudatis dudum attrectabam, cohæsiõ obtinebat aliqua; disjungebam, & tenuius, quod præpollerat in superiore experimento, in hoc item erat præpollens; ipsum ab hac disjunctiõne electricum defectu apparebat utrinque, & vitrum socium apparebat utrinque electricum excessu, inverso vitro tenuiore, electricitates a conjunctione, & disjunctiõne ibant in contrarias, inverso socio crassiore manebant eadem.

**XXVII.** Post hæc capiebam experimentum idem, sed denudabam vitrorum facies superiores; his juncta vitra, nec quatiebant, nec cohæsiõnem monstrabant satis manifestam; sejuncta quatiebant ambo, sed tenuius validius; iterum juncta dudum attrectabam, atque tum a divulsiõne vitrum tenuius erat utrinque electricum excessu, crassius utrinque defectu; inverso tenuiore, electricitates mutabantur in contrarias; inverso alio manebant eadem.

**XXVIII.** Porro in primo experimento vitrum, quod contrarias electricitates acceperat in faciebus oppositis, quodque adeo habebat vim explodendi, ipsum erat præpollens; in experimento altero, quod sejunctum ab alio, aut solum explodebat, aut explodebat alio validius, ipsum erat præpollens. Idem

constat in tertio hoc experimento; quod nempe vitrum explodit alio validius, ipsum, quam habet in interiore sua facie electricitatem, eandem determinat in facie sua exteriori, & contrarias determinat in faciebus ambabus vitri focii (XXVI, XXVII.)

XXIX. Pergebam experiri vitris iisdem similiter electricis a catena, sed unius superiorem, alterius inferiorem faciem denudabam; hoc illi imponebam, continuo cohaesio obtinebat aliqua; attrectatis extimis indusiis, quatiebar, atque ab hac explosione cohaesio invalescebat; divellebam, & attrectabam seorsim singula vitra, vitrum tenuius quatiebat me mediocriter, aliud non item: juncta faciebus iisdem contrariis cohaerebant aliquomodo. Quare cum in experimento hoc tertio aptantur altera alteri contrariae vitrorum facies, similia iis obveniunt, quae in experimento secundo, ubi duo vitra fiunt junctim electrica.

XXX. Ceterum quales in secundo, tales in hoc experimento contingunt post explosionem electricitatum oscillationes, praesertim cum experior, ut in num. XXIX. tum quae pendent a sola vitrorum conjunctione, & disjunctione, tum quae cum iis sese componunt, & efficiuntur ab inversione vitri praepollentis.

XXXI. Vitri autem praepollentis (XXIX) vim aliter etiam experiebar. Posteaquam jam semel sejuncta adegeram ad explosionem, iterum jungebam, tum iterum disjungebam, atque vitrum tenuius iterum indutum explodebat aliquomodo; quod de novo, & tertio adhuc experiens aliquam adhuc percipiebam commotionem: vitrum autem crassius, quum



quum in ipso similiter experiri pergerem, nullo me-  
unquam commovebat modo.

#### EXPERIMENTUM QUARTUM.

XXXII. Hactenus de vitris duobus; pauca nunc ad-  
dam de uno. Vitrum rite indutum, & elec-  
tricum a catena summo prehensens angulo sus-  
pendo, tum diversa confector, quæ existunt phæ-  
nomena, indusia attrectando, divellendo, divellendo  
digitis, aut divellendo staminibus sericis; eaque  
omnia experior tum ante, tum post explosionem.  
Itaque, si alternatim attrecto indusia vitri electrici,  
alternatim ignem haurio a facie redundante, immitto  
in carentem; & copia ignis quaque attrectatione  
hausti, aut immissi respondet summæ capacitatis  
indusiorum, et magnitudini electricitatum abso-  
lutarum residuarum; eaque lege ignis hauritur, &  
immittitur alternis his attrectationibus, ut contices-  
cat electricitas omnis in facie attrectata, reviv-  
iscent in facie opposita.

XXXIII. Si indusia opposita attrecto simul ambo,  
explosio fit; intereaque indusia vitro adhærent va-

\* Ceu si indusium faciei unius vitri communicet cum uno ho-  
mine sejuncto a sole, aut cum duobus, aut cum tribus similiter  
sejunctis, aut si communicet etiam indusium faciei alterius cum  
uno, duobus, tribus, &c. hominibus, aut corporibus aliis defe-  
rentibus: copia ignis, quæ alterna quaque attrectatione immittit-  
tur in faciem carentem, aut hauritur a redundante, proportionem  
respondet summæ capacitatis & indusiorum chartaceorum, &  
hominum, aut corporum aliorum, quæ cum una, aut ambabus  
faciebus communicant; atque prout major est summa earum ca-  
pacitatum, minore attrectationum numero exhauriuntur absolutæ  
oppositarum facierum electricitates.

lidijs;

lidius; & dum pergo attrectare, explosio, & adhæſio conſummantur.

XXXIV. Pergo nunc induſia divellere ſtaminibus ſericis, ut, ſi quam ipſa a divulſione habent electricitatem, retineant; atque ante exploſionem facies quæque vitri ea cautione denudata electricitatem oſtendit ſibi contingentem, induſium quodque electricitatem pariter reſpondentem demonſtrat, & ſuæ capacitati proportionalem<sup>f</sup>; dico autem contingentes electricitates, quæ datæ faciei ante exploſionem conveniunt ex theoria Frankliniana.

XXXV. Si poſt exploſionem ſtaminibus ſericis induſia divello, facies vitri ambæ electricitatem monſtrant, contingentem faciei ultimo denudatæ, induſia demonſtrant contrarias; ipſaſque iterum ſuæ capacitati proportionales.

XXXVI. Nunc induſia digitis divello; quæ res non facit ſolum, ut ſentiri non poſſint electricitates induſiorum, ſed aliquam præterea infert mutationem iis electricitatibus, quæ apparent in faciebus nudis, cum per ſtamina ſerica induſia divello. Itaque ante exploſionem cum digitis divello induſium unum, ambæ facies electricitatem oſtendunt eandem, et contrariam illi, quæ contingeret faciei ultimo denudatæ.

XXXVII. Sed poſt exploſionem cum digitis induſia divello, tum utraque facies electricitatem demonſtrat eandem, ſed contingentem faciei ultimo denudatæ; atque hiſce in divulſionibus induſiorum, quæ aut digitis ſiant, aut ſtaminibus ſericis, oſcil-

<sup>f</sup> Loquor hic de capacitatibus, quas habent induſia chartacea ſola; etenim unice circa eas capacitates expertus ſum.

lationes electricitatum aliquæ facile observari possunt. Unam hic ego attingam, quæ accidit in hac ultima experimenti hujus parte. Vitri, quod adagi jam ad explosionem, latus unum apprimo stomacho meo, lævaprehendens latus oppositum, tum dextera divello indusium inferius, continuo pollice lævæ admoto indusio superiori, existit scintillula; divulsum indusium inferius iterum admoveo, superius scintillam exhibet aliam, sed primæ contrariam; iterum illud divello, iterum hoc scintillat, uti vice prima; illud rursus admoveo, rursus hoc scintillat, uti vice altera, &c. scilicet, si alternatim divello, & admoveo indusium faciei defectu electricæ, in divulsione indusium superius accipit ignem, effundit in admotione; si divello indusium faciei, quæ ante explosionem erat excessu electrica, indusium aliud inter divellendum effundit ignem, accipit inter admovendum. Quæ omnia penicillo, & stellula, aut motibus electricis confirmavi.

XXXIII. *Specimen Historiae Naturalis Volgensis.* Auctore J. R. Forster.

Read June 18,  
1767.

**A**NTEQUAM de quolibet genere corporum naturalium circa Volgam flumen agere possim, necessarium duxi quaedam circa loca & situm locorum praemonere.

Regio cujus historiam naturalem sum descripturus, est inter gradum 52 & 48 latitudinis borealis, estque sita ex utraque ripa fluminis Volgae; id flumen ad Saratoviam circiter milliaris Anglici latitudinem habet; at tempore verno, dum nives per universam Russiam dissolvuntur, aqua ingens incrementum capit, & non raro septem novemve pedes Anglicanos surgit, & tum temporis in ripa orientali per duorum triumve Anglicorum milliarium spatium diffunditur, et usque ad altiorem ibi insurgentem planitiem ingentis deserti late vagatur, prataque & filvas ibi foecundat. A mense autem Maio usque in Novembrem vel initia Decembris, quo scilicet tempore gelu constringitur, flumen sensim decrescit. Omnis ripa occidentalis Volgae per plusquam 260 milliaria Anglica est montosa; montes assurgunt in littore a 30 ad 60 orgyas Russicas, quarum quaelibet est 7 pedum Anglicorum; inde duorum triumve milliarium Anglicorum spatio a flumine, alia series montium, paene ejusdem altitudinis procurrit, eodem tenore quo flumen: & in horum montium dorso frequens est silva plurimum ex pulcherrimis constans quercetis ab ipso iugo  
horum

horum montium ingens planities, protenditur, ad usque flumen *Khoper* (*Choper*) quod in *Tanaim* influit nisi qua, hinc inde, rivis quibusdam dividitur. Omnis superficies harum regionum tecta est ad duorum triumve pedum altitudinem, terra admodum nigra, pingui & ad miraculum usque fertili; sub ea nil nisi marga & lapides margacei cretacei reperiuntur, ad ipsam usque flumen *Volga*. ~~Fluvius~~ <sup>Fluvius</sup> ~~idem~~ <sup>idem</sup> ~~riparia~~ <sup>occidentalis</sup> innumeris intersecta est convallibus, per quas rivuli & fontes, quorum pauci sunt perennes, ex propinquis defluunt montibus. In omni hac regione gramina & variae plantae primo vere ad duos tresve pedes rapidissime crescunt. Ingravescente aestu sensim siccantur herbae in vastis campis, & dein ab incolis, qui ~~non~~ <sup>non</sup> ~~admodum~~ <sup>admodum</sup> reperiuntur, primo vere igni comburuntur, maximo regionis detrimento; quia humiditas ex solo expellitur, & terrae crusta veluti testacea inducitur; ut taceam salia, quae hinc in hac ipsa ~~terracina~~ <sup>terracina</sup> ~~superficie~~ <sup>superficie</sup>, nimia sunt, & hyeme certe frigus, ~~intendunt~~ <sup>intendunt</sup>, aestate vero nimiam procreant siccitatem; praeterea & totae silvae flammis vento agitatis non raro intenduntur & pereunt maximo damno regionis hujus adeo frigidae.

Ripa orientalis *Volgae* planem ovum orbem nobis sistit. Ab ipso flumine per duo triave milliaria Anglica littora cincta sunt pratis amoenissimis, & silvis, fruticetisque varii generis: innumeri rivuli ex *Volga* derivati, paludes & lacus, haecce prata intersecant. Dein ~~supra~~ <sup>supra</sup> ~~haec~~ <sup>haec</sup> ~~prata~~ <sup>prata</sup> ad 30 pedum ~~altitudinem~~ <sup>altitudinem</sup> ~~usque~~ <sup>usque</sup> ~~tant~~ <sup>tant</sup> ascendendi, ex quorum summitate vastus ~~campus~~ <sup>campus</sup> & planities immensa ad *Yaiki* usque ripas protenditur ad 130 ~~vel~~ <sup>vel</sup> 150

milliaria Anglica. Solum in pratis ex argilla, arena, marga, & putrefactis vegetabilibus constat. Planities superior nil nisi mera argilla est, & maxime tritici millique ferax. In medio harum regionum est planities arenis cooperata & ericetis, in qua si vix ad pedis unius alteriusve altitudinem arenam eruere volueris, ingentem vim habebis aquae dulcis: praeterea innumeris in locis sunt lacus aquae salsae, rivulique amarulentam fassamque habentes aquam; imo & plures illic sunt lacus qui stratis salis muriatici 3 circiter pollicum crassitudine sunt lecta, ad plusquam decem duodecimve pedum altitudinem; & si hic inde rivuli aquae dulcis per valles quosdam defluunt, non tamen ubique aqua supra terram fluit, sed saepius per arenas & margacea strata transit, & post aliquot demum milliarium intervallum, denuo fluere incipit.

Coelum his locis plerumque sudum & serenum est. Hyeme intensissimum frigus, multaeque decidunt nives, circa mensis Novembris finem & per Decembrem: at fine Martii vel initio Aprilis subito, aëris temperie nives liquefunt herbae ubique luxuriant, & brevi tempore ad cubiti altitudinem succrescunt, & inde uno tenore aër usque ad summam & poene intolerabilem aestum incalescit, adeo ut gradum 103 $\frac{1}{2}$  thermometri Fahrenheitiani celeberrimus Lorchius Astrachani observaverit: & ego circiter sub gradu 51 degens mensibus Maio, Junio, & Julio 1765 in thermometro de Lilliano gradum 97 observavi, qui circiter cum 93 $\frac{1}{2}$  thermometri Fahrenheitiani coincidit.

Praeterea id precor cuivis legenti sequentes observationes animo obversetur, me non tam soluisse  
omnis

omnis numeris perfectam historiam naturalem harum regionum scribere; huic enim operi, nec tempus brevissimum, quo in iisdem oris mihi versari contigit\*, nec vires, ob reliquas mihi demandatas maximi momenti occupationes, nec ipse parvus tenuisque, quod scio ingenii mei modulus suffecissent; sed potius primitias historiae naturalis, me orbi erudito, ea qua par est animi modestia, offerre.

Naturae sequar ordinem in exponendis rebus; ab inanimatis, ad vegetantia indeque ad viventia processurus, in plantarum denominatione in animalium dispositione celeberrimi Linnaei editos libros, methodumque undique receptam, adhibiturus sum.

## REGNUM MINERALE.

### (A.) T E R R A E.

#### A. ARGILLA.

\* *Coloris lutei.* Omnis regio trans Volgam, quam late desertum inter Volgam Gaïkum, Samaram & mare Caspium patet, nil nisi huius generis argilla est, at tritici milique feracissima. Eadem argilla hinc inde & ex hac parte Volgae reperitur prope Saratoviam, prope coloniam Palatinam, ut & prope colonias ad rivulos dictos *Lesnoi Karamysk*, *Sosnosa*, *Ylafla*, & *Koola lynka*.

ß *Coloris rubicundi*, particulis ferri impraegnata prope Volgam ad rivulum *Ghmielofka*, ut & circa *Tarlyki* minoris ripas trans Volgam.

\* Scilicet à mense Maio ad initium Sept. 1765.

*γ Rhomboïdalis lamellosa lutea.* Haec argillae species in torrente qui Saratoviam urbem transit, in determinato profundo reperitur.

*δ Rhomboïdalis lamellosa nigricans.* Tactu mollißima pinguis & laevis sub argillae num. *γ.* memoratae strato invenitur eodem in loco; cum acidis effervescit, & plurima testacea in ea reperiuntur.

#### B. MARGA.

*α Viridescens, pinguis,* in colonia Palatina sub humo nigricante, reperitur maxima in copia.

*β Marga cinereo albicans cretacea,* ibidem eodem in loco effossa.

#### C. CRETA.

Circa villam Zolotoi prope Volgam reperiunda; incolae ea utuntur ad dealbandos parietes.

#### D. HUMUS NIGRICANS PINGUIS.

Cis Volgam omnis late regio a Pensa urbe ad Tanais viciniam ad duorum triumve pedum altitudinem hac humo tecta est: & uti facile ex adpectu dijudicari potest, orta est ex vegetabilibus per innumeras annorum series, in his locis quotannis putrefactis.

#### E. TERRÆ MINERALES.

*α Arena martialis,* prope *Ylastam* in vicina Canalis ab Anglo Perry effossi, ad jungendum Tanaim Volgae, item prope *Medveditzam*, camque influentes rivulos *Tersam*, *Dobrynkam*, & *Burluk*.

*β Terra sale & hepate sulphuris mixta* in fundo lacus Yeltoniensis salis, saepe inter strata salis reperitur.



γ *Argilla sale communi impraegnata.* Cis Volgam prope Saratoviam, ad montes Accipitrinos (*Sokolowe gori* vulgo) & trans Volgam in ripa minoris Tarlycki & plurium minorum rivulorum falsam aquam vehementium, statim ex magno proventu Salicorniae, Salsolae & Anabaseos dignoscitur.

δ *Terra selenitica*, in torrente qui urbem Saratoviam transit in *argilla rhomboïdali, lamellosa, nigricante* reperitur.

F. ARENA.

α *Glarea mobilis* ad littora Volgae orientalia praefertim reperitur.

β *Glarea argillosa*, hinc inde in pratis, trans Volgam.

γ *Sabulum particulis minoribus*, spathi, quarzi, & micae compositum. In medio vasti deserti trans Volgam, campus ingens hac arena coopertus in longum 140 in latum 50 miliaribus patens, & *Ryn* appellatus, a Volga 100, Yaiko 60 circiter, a mari Caspio autem 120 miliaribus Anglicis abest.

(B.) LAPIDES, in universum admodum rari.

α *Quarzosi*, in summis jugis montium cis Volgam reperiuntur integrae rupes ex hoc lapide compositae. Magnae satis molis & nomine aurium (*Ushy*) appellatae, prope Dmibresok urbem, trans Volgam, ultra 30 mill. Angl. conspicuae sunt.

β *Cosarenarius* in summis montium jugis ibidem.

*Calcarius,*

γ *Calcareus*, petrefactis testaceis plenus, in prae-rupta Volgae ripa prope Saratoviam, in strato duorum circiter pedum, a summo montis jugo 10 circiter pedibus.

δ *Lapis margaceus*. Omnia Volgae littora omnesque citiores partes nil nisi strata hujus lapidis margacei habent, qui nec calci faciendae bonus, nec in aedificiorum usum cedit, quia aëri expositus brevi dissolvitur.

ε *Tophus sale plenus*, in littore lacus Yeltoniensis.

*Nota.* Trans Volgam ne parvae quidem molis lapides, exceptis his tophaceis, reperies.

### (C.) INFLAMMABILIA.

1 *Gagates* 120 milliaribus Anglicis ad Volgae ripam supra Saratoviam repertus.

2 *Pyrites sulphureus* purus nudus 26 mill. Angl. supra Saratoviam in australi ripa *Tchardymi* fluminis 6 mill. Angl. a Volga reperitur; & totus mons ex hoc pyrite versus Africum (S. W.) ad Kurdyum flumen usque protenditur. Decem librarum pondo, dant per destillationem 16 libras sulphuris residuum vitriolo martis, & partibus martialibus est impraegnatum.

### (D.) SALIA.

1. *Sol commune.*

Ingentem salis copiam maximae Russici imperii parti sufficientem praebet lacus *Yilton* a Russis, a Khalmuccis autem *Gelton-noor*, appellatus.

Is

Is lacus trans Volgam a Saratovia 160, a Dmitrefok urbe circiter 80 Angl. mill. abest. A campo arenoso Ryn supra memorato 20 circiter mille passibus remotus; 10 in latum & sedecim in longum, circuitu vero mill. Angl. patet. Plures in eum incidunt rivi falsi. Aqua vix ultra 4 pedes Anglicos superfusa, intense salsa, coloris rubicundi, ab argilla ex montibus ut credo defluente, forte & ab alia causa, quam ignorare me fateor: relatum mihi enim est, lacum mense Augusto, post intensissimos calores, maxime rubere. Tertia circiter parte mill. Angl. a littore incipiunt strata salis per omnem lacum extensa, 3 circiter pollices crassa; haec strata in medio lacu ad insignem altitudinem reperiuntur. Anno 1746, quum primum hoc sale uti inciperent, curribus per lacum uti per glaciem commeabant, salem vecturi: sed curiosius inquirentes, quotnam strata salis in lacu essent, tandem aquae sub stratis inclusae locum dedere prorumpendi, & ex eo tempore aqua lacui superfusa est.

Mille vel 1200 homines lintribus circiter 400 vel 500 a fine Maii mensis, in altum Angl. circiter milliare a littore evecti in aquam descendunt & ope ferri conto impacti, strati salis partem aliquam a reliquo revellunt, trium quatuorve pedum plerumque magnitudine, & in lintrem tollunt, dein lintre onerato unus in littus cum sale abit, reliqui alium lintrem replere conantur. Postero die antequam ad laborem redeant, salem in littore malleis ligneis comminuunt, & aqua lacustri eluunt, est

est enim maximopere impurum. Quotannis ad 120 millena millia librarum salis a Maio ad Augustum eruunt, & 6000 vel 8000 bobus Saratoviam & Demetriopolim (Dmitrefok) ad Volgam in Imperatricis devehunt horrea, praeterea innumeri equi, carris juncti, salem in usum populi in vicinia exportant. Pro 40 libris salis, pro labore, operariis, & iis qui salem vehunt in universum Saratoviae  $4\frac{1}{2}d.$  Demetriopoli (Dmitrefok) tantum  $2\frac{1}{2}d.$  monetæ Anglicæ ex publico rependunt. Deinde autem salem per omne imperium Russicum navibus distribuunt & carris. Tum vero omnes unatâ librâ  $\frac{1}{2}d.$  Angl. pretio, emere possunt, ex publicis horreis, quovis in loco. Ingens copia *salis mirabilis*, inter strata salis passim reperitur, & licet maximam ejus partem supradicto modo eluant, remanet semper aliqua ejus pars, quæ efficit, ut hoc sale minus bene in aspergendis utantur carnibus, præsertim in usum nauticum, ideo in imperatoriae classis usum quotannis Hispanum important salem. Præter hunc lacum sunt & plures alii sale pleni, versus mare Caspium, in vicinia Astracani; sed eo sale uti vetitum est, ne in detrimentum publicorum reddituum eum eruerent; id vero inde incommodum ortum, ut piscatui nunc minus sint intenti populi Caspio mari vicini, salis nempe pretio duplo majori, quam antè fuerat, factio.

2. *Sal mirabile* uti supra memoravimus inter strata communis reperitur, in eodem lacu Yeltonenti;

*Alumen.*

Urbem Saratoviam transit rivulus in praerupta valle, in eundem a parte septentrionali influunt fontes tres, quorum aqua saporis est maximopere adstringentis; cum ea quaedam experimenta institui, ut nempe potui; non enim in hisce oris in promptu sunt omnia ad ejusmodi experimenta rite instituenda.

*α* *Odorem* nullum in hac aqua observare potui.

*β* *Visui* aqua se offert limpidissima; nec per complures dies asservata mutationem ullam subiit.

*γ* *Sapor* intense stypticus, adstringens, amarulentus.

*δ* *Media aestate*, frigidissima erat dum ex fonte prodiret.

*ε* *Solutio argenti in ∇*, aqua diluta, & huic aquae affusa, eandem subito turbidam, mox lactei prorsus fecit coloris: mox coagulatae in aqua videri poterant partes, quae sensim subsidere videbantur; superior pars hujus coaguli nigricantis erat coloris.

*ς* *Affuso Sp. Sal. Amoniae*, similiter coagulum in aqua videri poterat.

*ζ* *Ferrum politum & laevigatum* aquae injectum nigrum induit colorem, & in summa aquae superficie, ferro adhaesit ochra.

*θ* *Argentum laevigatum*, aquae immissum nigrum induit colorem, praeterea splendorem exuit.

*η* *Solutio ∅ in ∇ affusa*, coeruleo colore eam tinxit, praeterea vero nil mutatum.

1. *Albumen ovi* aquae immixtum nil mutavit.
2. *Tinctura quaedam coerulea*, mihi falso pro *Syrupo* violarum vendita, (erat enim tinctura Flor. Aquilegiae, aquae admixta,) nullam subiit mutationem.
3. *Solutio Mercur. sublimati* aquam aeque limpida ut antea reliquit.
4. *Infusione Gallarum* affusa, nil mutatum.
5. *Solutio Sal. Tartari* aquae adfusa lacteum colorem ei induit, dein & coagulatae partes viscae.
6. *Solutio Sacchar. Saturni* aquam minus limpida fecit, & postero die partes coagulatae viscae.
7. *Solutio Aluminis* affusa nil mutavit.
- Ex hisce satis apparet Aluminis aliquantum huic aquae inesse; praeterea visum est mihi partes sulphureas aquae admixtas esse; sed quia plura experimenta instituere mihi non licuit, utique aquae hujus reliqua ingredientia & proportionem observare nequii.
8. *Vitriolum Martis* in Pyrite supra descripto lit. C. n. 2. inesse, certissimum indicium fecit lixivium residui post distillationem *Aris* evaporatum.

### (E.) M I N E R A E.

In omni hac regione hactenus, pennis, ferri minerae sunt inventae.

- a. *Minera ferri cavernosa*, cujus 40 librae dant 16 libras ferri optimi, magis scoriae quam ferri minerae similis, at metalli dives est, & facillime liquefcit, adeo ut eo gradu ignis, quo aliae minere vix calcinantur, haec nostra jam liquefcit.

In

In superficie terrae inter arandum copiose reperitur in agro coloniae Palatinae. In deserto versus Choper fluvium magna in copia obvium est Ignem certissime jam passa est minera, ideo forte a monte ignivomo quondam eructata.

*b Minera ferri lamellosa, mixta arenae ferrariae, ex fodinis ferri, circa montes, qui imminent rivulis, Tersa, Dobrynka, & Burluk. Ferrum dant durum & non magni pretii, ex minera refractaria. Ejusdem generis minera ferri, circa fossam reperitur, quam Anglus Joannes Perry, jussu Imperat. Petri I, ad jungendam Volgam & Tanaim duxit.*

#### (F.) PETREFACTA.

Plurima Ammoniorum, Mytulorum, Terebratularum, Chamitarum, & Pinnae marinae exemplaria circa Saratoviam in lapide calcario & in argilla tessellata nigra supra descripta reperiuntur.

#### REGNUM VEGETABILE.

Plus quam septingentas in itinere meo collegeram herbas; sed plaustris, quibus cum reliquis sarcinis & herbaria mea imposita erant, aquam profundam transeuntibus, semina & herbaria quoque madefacta sunt, nec primis statim observavi diebus, dein irreparabilem jacturam me passum esse vidi, omnes enim plantae mucore corruptae, prorsus abjiciendae erant: & cum in chartas hasce bibulas, pro restaurando damno alias collegissem herbas, & has mucor ex charta propullulans infecit, itaque nonnisi reliquias

harum plantarum & paucas bene conservatas retuli, ex itinere. Hinc cuilibet patebit nonnisi specimen, admodum pusillum heic florae Volgensis offerri. In determinandis plantarum nominibus, Clariss. Falckius, Prof. Botanices in Horto Pharmaceutico Imperiali, pro insigni amicitia qua me prosequitur, me plurimum adjuvit, quod grata admodum mente publice fateor.

### DIANDRIA MONOGYNIA.

1. *SALICORNIA caule geniculato*; in apicibus ramorum sunt clavae floriferae articulis cylindricis, ex cujusvis articuli commissura prodeunt sex flores; in cavitate rotunda, prominet stylus, & adsunt duo stamina cum antheris.

2. ———— *caule tereti sine geniculis*; in ramorum apicibus sunt clavae floriferae articulis tetragonis rhomboidalibus; ex cujusvis articuli commissura prodeunt quatuor flores, ad rhomborum apices; stamina cum antheris duo, stylus minimus.

*Nota.* Quia antherae ob staminis tenuitatem sunt maximopere flexae, ipse humi positus oculo armato innumeros flores accuratius observavi, & in omnibus, duo stamina & unicum stylum reperi; itaque hae salicorniae species differant, in his regionibus ab iis, quas alii Botanici alibi cum una vel pluribus observaverunt antheris. Florent mense Julio & Augusto in locis humidis, prope lacus & rivulos salfos trans Volgam ad lacum Yelton, & in vicinia Yerooslani fluminis.

3. *VERONICA incana*, foliis oppositis superioribus sessilibus, integerrimis, inferioribus & radicalibus crenatis.



crenatis & petiolatis. Floret mense Junio locis fic-  
cis montosis cis Volgam.

4. ——— *longifolia* floret a Julio in Augustum  
ubique cis Volgam in campis & sylvis.
5. GRATIOLA *officinalis*, floret a Julio in Septem-  
brem in pratis & locis humidis, ubi sese per stolones  
reptantes propagat.
6. SALVIA *nemorosa* floret a Maio per totam aesta-  
tem locis aridis.

#### TRIANDRIA MONOGYNIA.

7. VALERIANA *officinalis* floret m. Jun.
8. IRIS *pseud. Acorus* floret m. Junio in aquis.
9. ——— *Sibirica* floret m. Maio & Junio in pratis  
humidis ad Volgam.

#### TRIANDRIA DIGYNIA.

10. PHALARIS *erucæformis* floret Jun. in campis cis-  
Volgam.
11. STIPA *pennata*, aristis longissimis floret m.  
Junio, seqq.

#### TETRANDRIA MONOGYNIA.

12. SCABIOSA *arvensis* floret m. Julio seqq.
13. ——— *ochroleuca* floret in campis & ad sege-  
tes m. Jul. seqq.
14. GALIUM *rubroides* floret m. Junio.
15. ——— *palustre* floret m. Junio circa rivulos  
in silvis.
16. ——— *verum* ad rivulos & fontes m. Jun.
17. ——— *Aparine* ad sepes villarum m. Jun.

18. RUBIA

18. *RUBIA peregrina* foliis quaternis; baccæ duæ monospermae, semina umbilicata; floret m. Jun. ad ripam Dobryn, Kae & Yerooslan.
19. *PLANTAGO major*, ad vias & semitas passim, fl. m. Jul.
20. ————— *media*, in montibus soli expositis, fl. eodem tempore.
21. *SANGUISORBA officinalis*, copiose in pratis, fl. m. Jun. seqq.
22. *ALCHEMILLA vulgaris* in pascuis humidis.

## TETRANDRIA DIGYNIA.

23. *ANONYMOS* calyx exterior triphyllus, interior quadriphyllus. Corolla nulla, styli duo plumosi, antherae majores sagittatae, foliola linearia fasciculata, flores alares, radix lignosa ut & tota planta, quae raro pedem & dimidium excedit. Ob seminis defectum determinare non potui, quo potissimum sit referenda haec planta; floret Julio & Aug. locis aridis in deserto trans Volgam prope Yerooslanum flumen.

## PENTANDRIA MONOGYNIA.

24. *MYOSOTIS scorpioides* locis humidis floret m. Junio.
25. ————— *apula* fol. hispida, racemis foliosis locis aridis m. Jul.
26. *LITHOSPERMUM officinale* floret Maio in montibus & locis apricis circa Koordyoom & Saratoviam.

27. *ONOSMA*,

27. ONOSMA, *simplicissima* fol. lanceolato—lineari-  
bus, confertissimis, asperis, antheris sagittatis, quae  
mihi connatae videbantur; fl. m. Jun. locis altiori-  
bus ficcis.
28. ECHIU *Italicum* flor. m. Junio in collibus.
29. LYSIMACHIA *vulgaris* floret m. Junio locis hu-  
midis.
30. ————— *nummularia* floret m. Augusto in  
pratis.
31. CONVULVUS *arvensis* fl. m. Jul. inter segetes  
passim.
32. ————— *sepium* inter fentes & dumeta fl.  
Julio.
33. POLEMONIUM *coeruleum* in sylvis & pratis floret  
m. Maio.
34. CAMPANULA *rapunculoides* habitat in montosis  
regionibus fl. m. Junio.
35. ————— *Trachelium* in sylvis fl. eod. temp.
36. VERBASCUM *Thapsus* fl. m. Jun. locis ficcis.
37. ————— *Lychnitis* flor. m. Jun. in campis.
38. ————— *nigrum* ad ripas rivulorum & fon-  
tium m. Jun. seqq.
39. ————— *Phoeniceum* florib. odoratissimis fl.  
m. Maio & Junio in campis desertis prope Pe-  
trofsk, & prope Sosnofkam in Volgae ripa.
40. SOLANUM *Dulcamara* fl. m. Junio locis um-  
brosis.
41. ————— *nigrum* fl. in Jun. in arvis & ad sepes.
42. RHAMNUS *Frangula* fl. m. Maio in humidis ad  
rivulos.
43. EVONYMUS *Europaeus* fl. m. Maio in sylvis ad  
rivulos.

44. *RIBES nigrum* fl. m. April. & Maio in sylvis passim.

## PENTANDRIA DIGYNIA.

45. *ASCLEPIAS Vincetoxicum* locis humidis & umbrosis fl. m. Jun. seqq.  
 46. *SALSOLA Kali* foliis subulatis, spinosis, caule decumbente, calycibus axillaribus, marginatis.  
 47. ————— *rosea*.  
 48. ————— *prostrata*.  
 49. ————— *hirsuta*.  
 50. *ANABASIS foliosa*. Omnes hae plantae reperiuntur ad rivulos & paludes salias, & locis sale communi imbutis, prope Saratoviam & trans Volgam in deserto prope lacum Yelton & rivulos salios in hunc lacum sese exonerantes; fl. m. Julio.  
 51. *ULMUS campestris* in sylvis passim.  
 52. *ERYNGIUM planum* ad vias passim fl. m. Julio.  
 53. *HERACLIUM Sphondylium* in agris & campis passim fl. m. Jul.  
 54. *ANGELICA atro-purpurea* floret m. Jun. locis humidis.  
 55. ————— *sylvestris* floret m. Junio in umbrosis passim.  
 56. *CHAEROPHYLLUM sylvestre* fl. m. Jul. in umbrosis.  
 57. *AEGOPODIUM Pedagraria*, in sylvis & umbrosis passim fl. in Junio.

## PENTANDRIA TRIGYNIA.

58. *VIBURNUM Opulus* in sylvis & humidis floret m. Junio.

PENTANDRIA PENTAGYNIA.

59. *STATICE reticulata* } floret m. Jun. locis  
 60. ———— *speciosa* } aridis & passim sal-  
 61. ———— *tatarica* } fugine aspersis.  
 62. *LINUM perenni* floret m. Jun. & Jul. in pratis  
 prope Coloniam Palatinam.

HEXANDRIA MONOGYNIA.

63. *ALLIUM paniculatum*, floret m. Jun. in locis  
 desertis.  
 64. *FRITILLARIA Meleagris*, flor. April. in montibus  
 aridis ad Volgam.  
 65. *TULIPA sylvestris*, flor. m. April.  
 66. ———— *Gesneriana* ———— locis aridis pul-  
 cherrimis coloribus nonnulla exemplaria superbire  
 vidi, praesertim ad tumulos quosdam sepulchrales  
 in medio deserto 60 circiter mill. Angl. ab omni  
 habitatione remoto.  
 67. *ASPARAGUS officinalis*, in omnibus pratis ad Volgae  
 ripam copiose fl. m. Jun.  
 68. *CONVALLARIA maialis* flor. in sylvis m. Maio.  
 69. ———— *multiflora*.  
 70. ———— *bifolia*, fl. m. Maio in humidis.  
 71. *ACORUS Calamus*, in aquosis & paludibus passim.  
 72. *RUMEX Acetosa*, in pratis Jun.

HEXANDRIA TRIGYNIA.

73. *ALISMA Plantago aquatica* in paludibus ad  
 Hassiam & trans Volgam, in pratis m. Jul.

HEPTANDRIA MONOGYNIA.

74. TRIENTALIS *europaea*, fl. m. Maio in fylvis,

OCTANDRIA MONOGYNIA.

75. EPILOBIUM *angustifol.*  
 76. ————— *hirsutum*, } florent m. Junio lo-  
 77. ————— *tetragonum*, } cis humidis.  
 78. VACCINIUM *Myrtillus* in fylvis humidis m. Maio  
 fl.  
 79. ————— *Oxycoccus* in paludibus frequens;  
 & modo non omni tempore venum, asportantur  
 baccae Oxycocci, praesertim m. Jun. Ruffi eas  
 magna copia ingurgitant, contra calores; vel etiam  
 succum expressum, aqua & melle admixtum, pro  
 potu habent, quem palato admodum jucundum  
 esse, ipse expertus sum.

OCTANDRIA TRIGYNIA.

80. POLYGONUM *Bistorta* fl. m. Maio in pratis.  
 81. ————— *Convolvulus* inter segetes passim.

ENNEANDRIA HEXAGYNIA.

82. BUTOMUS *umbellatus* in palustribus m. Jun.

DECANDRIA MONOGYNIA.

83. ARBUTUS *Uva ursae* prope Twer in paludibus  
 jam in vicinia Volgae fl. m. Maio.  
 84. PYROLA

84. *PYROLA rotundifolia*, } florent m. Maio in  
85. ——— *minor*, } sylvis.

DECANDRIA DIGYNIA.

16. *SCLERANTHUS annuus* fl. m. Maio locis arenosis, radicibus hujus ut & *Potentillae reptantis* & *Tormentillae* adhaeret *coccus Polonicus*; & a mulieribus frequenter colligitur, aceto necatur, & venum asportatur.

DECANDRIA TRIGYNIA.

87. *SAPONARIA officinalis*, fl. m. Jun. locis ficcis.  
88. *DIANTHUS arenarius*, fl. m. Jul. & Aug. ad ripas Volgae.  
89. *CUCUBALUS tataricus*, } fl. m. Jun. locis aridis  
90. ——— *sibiricus*, } & altioribus.  
91. *ARENARIA saxatilis*, fl. m. Jun. in montibus ficcis ex lapide margaceo constantibus.

DECANDRIA PENTAGYNIA.

92. *SEDUM acre* floret m. Aug. locis arenosis.  
93. ——— *Telephium* locis aridis in deserto.  
94. *AGROSTEMMA Githago* inter segetes.  
95. *LYCHNIS Chalconica* prope Coloniam Palatinam copiose in sylvis & locis humidis.

DODECANDRIA MONOGYNIA.

96. *PORTULACA oleracea* ad ripam Yerooslani fluvii.

97. *LYTHRUM Salicaria* ad rivulos fl. m. Jul. Aug.  
pp.

DODECANDRIA TRIGYNIA.

98. *EUPHORBIA Peplus* fl. m. Julio in agris.  
99. ————— *helioscopia* ibid. eod.  
100. ————— *palustris* colossæa 4 & 5 pedum  
Angl. ad ripas Volgae & Occæ in pratis humidis  
fl. m. Maio.

ICOSANDRIA MONOGYNIA.

101. *AMYGDALUS nana* ; in campis desertis integræ  
sylvulæ occurrunt, fl. m. Maio.  
102. *PRUNUS Cerasus caproniana* in desertis cis Vol-  
gam, integræ ex ea sylvæ reperiuntur, fl. m. Maio.  
103. ————— *Acacia Germanor.* fl. m. Maio inter  
dumeta.

ICOSANDRIA TRIGYNIA.

104. *SORBUS aucuparia* in sylvis m. Maio.

ICOSANDRIA PENTAGYNIA.

105. *PYRUS Malus sylvestris* ; in sylvis hinc inde ar-  
bores majores reperiuntur, fl. m. Maio.  
106. *SPIRÆA salicifolia* herbacea, ad montium ra-  
dices, floret m. Jun.  
107. ————— *Filipendula* in campis humidis.  
108. ————— *Ulmaria* in pratis.

109. *SPIRÆA*



109. SPIRAEA *crenata* f. *tatarica* folia ovato-oblonga, alia apice crenata, alia integerrima, corymbus terminalis; flores albi; fl. m. Maio in campis desertis passim.

ICOSANDRIA POLYGINIA.

110. RUBUS *Idaeus* magna ingentique copia in sylvis provenit, & in montibus sylvestris.  
 111. ——— *fruticosus*.  
 112. FRAGARIA *Vesca sylvestris* & *pratensis* m. Maio.  
 113. POTENTILLA *Anserina* in pratis.  
 114. ——— *reptans* in campis passim argillofis foli expositis, coccum Polonicum alit.  
 115. TORMENTILLA *erecta* in prato sylvestri m. Maio floret, cocci Polonici ferax.  
 116. COMARUM *palustre* in locis humidis.

POLYANDRIA MONOGYNIA.

117. CHELIDONIUM *majus* ad sepes m. Maio.  
 118. PAPAVER *Rhoeas* inter segetes m. Junio.  
 119. NYMPHAEA *alba* } in flum. Ylaffa & in pa-  
 120. ——— *lutea* } ludibus copiose trans  
 Volgam.  
 121. TILIA *europaea* copiose in sylvis floret.

POLYANDRIA TRIGYNIA.

122. DELPHINIUM *Consolida* fl. a m. Jun. inter segetes.  
 123. ——— *elatum* inter fruticeta m. Jul.  
 124. ACCO-

124. *ACONITUM uncinatum*, fol. magnis, palmatis, lacinis magnis, flores pallide coerulei singulares in Querceto humido prope Arismaffium urbem, exitu m. Maii.

POLYANDRIA POLYGYNIA.

125. *ANEMONE sylvestris* fl. m. Maio & Junio in humidis & sylvestribus locis.  
 126. *THALICTRUM flavum* } flor. m. Jun. locis  
 127. ————— minus }  
 humid.  
 128. *TROLLIUS europaeus* fl. m. Maio in pratis.  
 129. *CALTHA palustris* ad ripas rivulorum in April.

DIDYNAMIA GYMNOSPERMA.

130. *AIUGA pyramidalis* fl. m. Maio in campis.  
 131. *SIDERITIS hyssopifolia* fl. m. Jun. in campis desert. cis Volgam.  
 132. *MENTHA aquatica* fl. m. Jul. palustribus.  
 133. *GLECOMAhederacea* in pratis copiose fl. m. Maio seqq.  
 134. *GALEOPSIS Ladanum* fl. m. Jun. in arvis.  
 135. *PHLOMIS Herba Venti* fl. m. Jun. in campis.  
 136. *ORIGANUM vulgare* fl. m. Jun. inter fruticeta.  
 137. *THYMUS Serpillum* fl. m. Jun. locis ficcis montosis.  
 138. *DRACOCEPHALUM sibiricum* fl. m. Jul.  
 139. *SCUTELLARIA galericulata* fl. m. Jul. locis humidis.

## DIDYNAMIA ANGIOSPERMA.

140. RHINANTHUS corollis labio superiori, recurvo emarginato; flores pallide lutei, alares in spica homomalla, fol. lanceolatis integerrimis, fl. m. Maio & Jun. in campis & sylvis arenosis subhumidis.
141. ————— *crispa* Galli fl. in pratis m. Jun.
142. EUPHRASIA *officinalis*, } In campis desertis fl.
143. ————— *Odontites*, } in Maio & Junio.
144. MELAMPYRUM *arvense* inter segetes Jul.
145. PEDICULARIS *comosa*, flore pallide luteo, m. Junio in campis.
146. ANTIRRHINUM *Linaria* fl. m. Jun. in campis.
147. SCROPHULARIA *nodosa* fl. m. Jul. ad. aquarum rivulos.
148. ORABANCHE major fl. m. Jun. in montibus.
149. DODARTIA *orientalis*, fol. integerrimis glabris linearibus, flor. caeruleis; fl. m. Julio in campis trans Volgam ad Tarlyk majorem fluvium. Cameli, equi, boves & oves Khalmeyccorum sunt ejus appetentissimæ, & ad radices usque demorsam comedunt; ideo magno labore & longissima excursionem demum locum intactum nactus sum, in quo exemplaria integra reperi ex quibus eam definire potui.

## TETRADYNAMIA SILICULOSA.

150. ALYSSUM *incanum* in campis fl. m. Jun. ,

TETRADYNAMIA SILIQUOSA.

151. SISYMBRIUM *Sophia* fl. m. Jun. in aridis.

MONADELPHIA POLYANDRIA.

152. SIDA; *calyx* tomentosus, coriaceus, quinquefidus, complicatus, *folia* magna, cordata, crenata, acuminata, tomentosa, petiolis folio longioribus; capsulae tomentosae, 12, 13, 14, calyce longiores, semina in qualibet capsula tria, reniforma, fl. m. Aug. ad Yerooflani ripam.
153. ALTHAEA *officinalis* flor. m. Jun. in campis humidis.
154. LAVATERA *thuringiaca* fl. m. Jun. inter fruticeta.

DIADELPHIA OCTANDRIA.

155. POLYGALA *vulgaris* } fl. m. Jun. in campis  
156. ————— *sibirica* } herbosis.

DIADELPHIA DECANDRIA.

157. SPARTIUM *complicatum* fl. m. Maio locis ficcis.
158. GENISTA *tinctoria* fl. m. Jun. & Jul. in campis herbidis patentissimis.
159. LATHYRUS *tuberosus*, } fl. m. Junio copioso in  
160. ————— *pratensis*, } pratis & campis.
161. VICIA *piciformis*, stylus villosus petiolis angulosis substriatis, stipulis sinuato-sagittatis, foliis ovatis, retusis, mucronatis, pedunc. multifloris imbricatis,

catis, flores ochroleuci ; crescit inter dumeta in ripa  
rivuli in Colonia Palatina.

162. ——— *dumetorum*, inter fruticeta, } m. Jun.  
163. ——— *Cracca* in pratis,  
164. GLYCYRRHIZA *echinata* fl. m. Jun. ad ripam  
Volgae & Dobrinkae in pratis.  
165. ——— *glabra*, integros ubique ac spa-  
tiosissimos campos occupat, fl. m. Jun.  
166. CORONILLA *varia* fl. m. Jun. in campis &  
pratis.  
167. HEDYSARUM *Onobrychis* fl. m. Jun. in mon-  
tibus margaccis.  
168. PHACA *alpina* fl. m. Jun. in campis.  
169. ASTRAGALUS *contortuplicatus* m. Jun. in  
campis ficcis.  
170. ——— *pilosus* in aridis locis fl. m. Julio.  
171. ——— *Syriacus*, in locis altioribus.  
172. TRIFOLIUM *Melilotus officinalis*, toti campi  
meliloto, fl. luteo & albo, occupantur.  
173. ——— *rubens* fl. m. Jun. in pratis.

#### POLYADELPHIA DIANDRIA. .

174. HYPERICUM *perforatum*, inter fruticeta.

#### SYNGENESIA POLYGAMIA AEQUAL.

175. SONCHUS *Alpinus* in campis altioribus fl. m.  
Jun.  
176. LEONTODON *Taraxacum* in pratis m. Maio.  
177. CICHORIUM *Intybus*, fl. m. Jul. in campis.  
178. ARCTIUM *Lappa* fl. m. Jun. ad sepes.

179. *EUPATORIUM cannabinum* fl. m. Junio ad aquarum rivulos.

## SYNGENESIA POLYGAMIA SUPERFLUA.

180. *TANACETUM vulgare* m. Jul. in campis & ad sepes.  
 181. *ARTEMISIA Abrotanum* ad Volgae & Occae ripam copiosissime.  
 182. ————— *Pontica* fl. m. Aug. in campis aridis.  
 183. ————— *Absynthium* in montibus ficcis.  
 184. ————— *vulgaris* ad sepes.  
 185. ————— *Dracunculus* in campis patentibus & ad montium radices.  
 186. *SENECIO Donia* fl. m. Jun. in pratis.  
 187. *INULA Helenium* copiose in pratis & ad rivulos m. Jun.  
 188. *CINERARIA Alpina*, & quidem ea varietas quam Jaquinus in fl. Vindeb. p. 287. *nudam* vocat, in montibus margaceis ficcis.  
 189. *ACHILLEA ptarmica* in pratis trans Volgam copiose m. Jun.  
 190. ————— *Millefolium* in campis ad segetes & vias passim m. Jul.

## SYNGENESIA POLYGAMIA SEGREGATA.

191. *ECHINOPS Sphaerocephalus* in campis ficcioribus.

## SYNGENESIA MONOGAMIA.

192. *VIOLA tricolor* passim in arvis & pratis trans Volgam.

193. IM-

193. IMPATIENS *Noli-tangere* inter frutices m.  
Jun.

GYNANDRIA HEXANDRIA.

194. ARISTOLOCHIA *Clematitis* in sylvis m. Maio  
flore.

MONOECIA TETRANDRIA.

195. BETULA *alba* fl. m. Apr. sylvar. decus.  
196. ——— *Alnus* fl. m. Apr. ad ripas aquarum.  
197. URTICA *dioica* fl. m. Jun. ad sepes.  
198. MORUS *Tatarica* habitat in Insulis Volgae, quo-  
tannis inundationi expositis circa Tzaritzinum, ubi  
integrae ejus sunt sylvae, sic & in omnibus insulis  
Tanais ab ostio ejus ad Tzaritzinum usque reperitur  
copiose. *Bombyces* foliis hujus mori educati, seric-  
um produxere tanti splendoris, adeoque bonum,  
ut nulla in re inferius sit serico Taurinensi. Impera-  
trix ipsa ex serico a me oblato, jussit fieri pulcher-  
rimas fascias, quas capitis ornatus causa admove-  
re dignata est.  
199. XANTHIUM *strumarium* ad sepes & vias  
passim.

MONOECIA POLYANDRIA.

200. QUERCUS *Robur*. Sylvae ad Volgam & in-  
feriora Tanais sunt maximam partem quercetum  
continuum, quod memorandum, quia quercus  
rarae in septentrionalibus Russiae. Incolae quer-  
no ligno in aedificando & in foco utuntur : juniores  
X x 2 quercus

quercus piscatores frequenter caedunt, ut cortice querno, additis veteribus ferramentis, retia nigro tingant colore, magno sylvarum damno: quamobrem ipse aliam in Borussia usitatam proposui methodum retia nigro induendi colore, nempe ut fumigentur, quod & magis conducatur retibus, & pisces allicere credatur.

MONOECIA MONADELPHIA.

201. PINUS *sylvestris*, hujus arboris solas quinque vidi ad villam Sosnofkam, quae hinc & nomen sortita est: praeterea defunt in ripis Volgae per plus quam 600 mill. Angl. a mari Caspio.

DIOECIA PENTANDRIA.

202. HUMULUS *Lupulus* arbores scandit in locis humidis.

DIOECIA OCTANDRIA.

- |                          |   |
|--------------------------|---|
| 203. POPULUS <i>alba</i> | } integræ sylvæ in pratis<br>inundatis, trans Volgam,<br>harum arborum. |
| 204. ——— <i>tremula</i>  |   |

POLYGAMIA MONOECIA.

205. VERATRUM *album*. Incolae credunt, id comesum pecoribus omnis generis mortale esse. Radicem siccatam & in pulverem reductam, vulneribus in tergo boum inspergunt, & hac ratione



- oestrum expellunt. Copiose fl. m. Maio in locis humidis circa Saransk & Pensam urbes.
206. *ACER Tataricum* fl. m. Maio ad ripas rivulorum & locis humidis.

## CRYPTOGAMIA FILICES.

207. *MARSILEA natans*. Folia plantae ovata, opposita, complicata; omnem superficiem foliorum tegunt glomeruli ex quatuor albis filamentis in apice veluti cirrheideis, compositi. Infra ramulos plantae, multae in aquam radices demittuntur; diversi hinc inde ab his radicibus reperiuntur glomeruli pilorum, qui calicis loco cingunt globulos tres, quatuor, quinque, septem, plures glomeratim adhaerentes, & stylis tenuissimis instructos, globuli maturi continent semina plurima albicantia ovata. Itaque nunc credo hac observatione me satisfacturum desiderio IH. *Linnaei*, qui genus *Marsileae* determinare promissit, si haec *Marsileae* species examinata fuerit. Integrae paludes trans *Volgam* hacce planta sunt tectae. Quae hic descripti nonnisi oculo armato vidi m. Aug.

## REGNUM ANIMALE.

## CLASSIS PRIMA. MAMMALIA TERRAE.

1. *CANIS familiaris domesticus*, inter rusticos frequens.
2. ——— *vertagus*, hic uti in omni Russia haec canum species pisces longissimis, & pedibus durioribus

subvillosis, videtur adaptata regioni nivibus hieme opertis, ideoque & tum ad venationem adhiberi possunt, quum reliqui vertagi nil proficiunt.

3. ——— *Lupus*, frequenter admodum & pecoribus damnosi sunt lupi, hisce in oris.
4. ——— *Vulpes*, ex utraque Volgae ripa frequentes; ipse catulum captum vidi trans Volgam ad Tarlyk flumen.
5. *FELIS Catus*. Feles ferae in sylvis frequentes dicuntur, a me tamen non visae.
6. *MUSTELA Lutra*. In paludibus & lacubus trans Volgam plurimas Lutras esse, relatum mihi est: vidi unam, mox se aquae immergentem.
7. ——— *Martes*, pelles earum a rusticis frequenter in urbes venum-afferuntur, non tamen magni faciuntur.
8. ——— *Putorius*, & harum pelles saepius venales.
8. *URSUS Arctos*, ad Galkam rivum prope coloniam, paulo ante meum adventum, quinque simul sunt visi; alius hieme Coloniam Palatinam placide transiit, & intactus sylvam vicinam attigit.

#### BESTIAE.

10. *SUS Scrofa*, a Rusticis heic satis magnae aluntur fues. Apri circa Astrachanum in arundinetis, prope Caspium mare admodum frequentes obvii.
11. *ERINACEUS Europaeus*, in sylvis & montibus reperiuntur.
12. *TALPA Europaea*, reperitur quidem, sed raro.

## GLIRES.

13. *LEPUS timidus*. Heic loci duae species leporum reperiuntur. Una hieme alba, exceptis auribus, aestate rufam griseumque lanam rursus induit; at haec species minor est, eorumque lana a pileariis usurpari nequit. Altera species omni tempore aequae ruffo-grisea, sed major, & lana earum a pileariis usurpatur. Ex sola Vorainia, quotannis hujus speciei ultra 60,000 pellium Moscuam & Petropolim dein devehuntur, unde postea navibus ad exteras oras praesertim in Angliam exportantur.
14. *Mus Marmotta*, in campis desertis inter Pensam Petroffk & Sarotof urbes ingens Marmottarum copia est; cauda pilosa admodum.
15. — cauda subabbreviata, auriculis subrotundis, pedibus brevioribus, palmis subtetradactylis, plantis pentadactylis, ventre adiposo terrae adpresso, corpore subtus pallide luteo, in dorso griseo lutescente, magnitudine excedit Rattum.
16. — *Rattus*, } in horreis & domibus  
17. — *Musculus*, } habitant.

## PECORA.

18. *CAMELUS Bactrianus*, tophis dorsi duobus. Una varietas colore fusco, altera pallide fusco-alicante; haec rarior. Khalmycci eis utuntur, & pretium eis 12 vel 16 aureor. (*Guinea* vulgo) constituunt. Khalmycci & Nogaiji Tartari prope Astrachanum ex lana camelorum neta & texta pannos egregios conf-

conficiunt, nostro Camelot simillimos, quos ipse vidi; sic & funes optimos ex his lanis torquent.

19. CARRA *Hircus*.

Tres ejus hic sunt obviae varietates.

1. *Capra vulgaris*, per totam Russiam eadem.
2. *Capra Kbalmyccica*, non nisi pedibus brevioribus distincta, ut & totum corpus praecedente multo minus.
3. *Capra Angorensis* in Orenburgensi praefectura apud Vvirgisos passim obvia; sic & curiositatis gratia Astrachani, ex Ghilano Persiae provincia, advectas haberi capras hujus speciei, ex testibus fide dignissimis audiui. Lanæ ex Persia advectas & a colonis Germanis, in filum netas & textrinae adhibitæ, vidi cum serica.

20. ——— *Sayga* vulgo Russis, relatum mihi eam in campis desertis patentibus frequentem esse; ipse non vidi, his locis; cornua vidi 6 uncias Gallicas paullulum excedentia, teretia annulata, recta in apice paullulum incurvata. Dein Petropoli, animal vidi, circiter duos pedes Parisinos, & quod excedit altum, coloris pallide admodum rufescentis, subtus albicantis.

21. OVIS. *Aries*.

Tres heic sunt varietates ovium.

- a *Russica vulgaris* quarum lana, pilis caninis propinquior, non magni pretii; & quod rustici nullam earum curam habent indies lana deterior fit; minores.
- b *Kbalmyccicae oves*, sunt Russicis multo majores, capita & nares incurvae, aures pendulae, pedes elongati, cauda lata, crassa, adiposa, ultra

ultra 30 libras pendens, lana in adultioribus pilosa rigida, in agnis undulosa splendens; a magnatibus Rusinae in vestes hibernas expectantur pelles agnorum, & magno satis pretio emuntur. Khalmycci pelles has perficiunt lacte, unde id commodi habent, ut non solum sint mollissimae, verum & madefactae & de-nuo siccatae non rigidae fiant. Oves usque ad 200 libras sunt ponderosae.

c *Tchercassicae oves* lanam Anglicae prorsus aequalem gestant. Hae oves circa Medvedizzam flumen haberi possunt. Quum Imperator Petrus anno 1722 arma in Persiam expediret, & Tchercassicas videret oves, edicto proposito iussit ut per omne Astracaniae & Casaniae regnum hasce oves solas alerent, unde quaedam huc pervenere; mortuo autem paucis post annis Imperatore edictum hocce prorsus neglectum: praeterea ad fluvium Kynel colonia est Tchercassorum, in Orenburgensi praefectura, quae has oves secum adduxit.

22. *Bos Taurus.* Boves Russorum sunt minores, Khalmyccorum majores, maximae ex Vorainia adductae.

23. *Equus. Caballus.*

a *Russici equi* sunt minores, duri & longinquis itineribus apprimis utiles.

b *Khalmyccici Equi*, paullo majores Russicis, duri, imprimis in longinquis itineribus utiles, gramen quodlibet, etsi durum, stramineum, frutices imo aequi bonique faciunt; triduo ad 400 milliaria Anglica cursu perficiunt. Venditi a Khalmyccis non raro per plusquam 100 Angl. milliarium spa-

tium per deferta ad suos redeunt dominos, saepe & Volgam tranant. Hieme sub nive pede cibum quaerunt.

*Nagayenses & Turcommannici & Bashkirici* maxime vero *Kirgisici Equi* multo his sunt validiores & majores, laboris feracissimi, aequae affueti ungula sub nive pastum quaerere.

*d Tchercaffici Equi*, *Khalmyccis* vix majores, nec forma praestantes, nam cervix eis rigida, & cum plurimis equis ea sit convexa, his ut plurimum est concava, at cursu longe omnium velocissimi, & aequae ut reliqui laboris feracissimi, triduo ultra 500 imo 600 mill. Angl. perficiunt.

*Caro equina Khalmyccis* pro cibo, pullus inter lautiora reponitur, lac pro potu, acescens, fermentans & destillatum inebrians, *Kumys*; bis destillatum *Arecca* appellatur, saporis admodum ingrati.

## CLASSIS SECUNDA. AVES.

### ACCIPITRES.

1. *FALCO*, *Melanaëtes*, *Aquila*, *Valeria*, cera pedibusque luteis. Ipse pullum hujus generis, plusquam duos pedes Anglicos longum vidi, circa Dobrynkam.
2. ——— *aeruginosus*, cera luteo-viridi, corpore griseo fusco, pedibus luteis, ubique passim circa Volgam obvius.

### FICAE.

## PICAE.

3. CORVUS *Corax*.
4. ——— *Cornix*.
5. ——— *Frugilegus*.
6. ——— *Monedula*.
7. ——— *Monedula tota nigra*, nec unquam caerulefescens, occipite incano, fronte, alis caudaque nigris; magnitudine monedulae vel parum minor. Omnes hi *Corvi* in sylvis ad Volgam frequentes.
8. ——— *glandarius*, Pica Glandaria.
9. ——— *Pica*. Pica varia.
10. GRACULA *Atthis*, corpore viridi, dorso caerulefescens, abdomine ferrugineo, pedibus rubris, magnitudine monedulae; trans Volgam ad Yerooslanum flumen vidi.
11. CUCULUS *canorus* in sylvis passim auditur.
12. PICUS *martius* } circa arbores putrescentes fre-
13. ——— *medius* } quens uterque.
14. MEROPS *Apiaster* gregatim trans Volgam ad Yerooslanum flumen victitat.
15. UPUPA *Epops* ad ripas rivulorum in fruticetis prope coloniam Palatinam a me visa.

## ANSERES.

16. ANAS *Cygnus* in lacubus ad Volgam frequens.
17. ——— *Cygnoides orientalis* s. *Anser Moscoviticus* rostro luteo cera magna nigra, occipite atro, sacco gulari; corpus griseo-luridum, albo mixtum, pedes luteo-rubicundi. Passim ut *Anser domesticus* alitur; tamen multo major est.
18. ——— *Anser*. Anseres feri innumeri in paludibus

trans Volgam degunt. *Anser domesticus* ubique alitur.

19. — *Anas moschata* fera & domestica, his locis habetur.
  20. — *Boschas major*.
  21. — *clypeata*
  22. — *platyrhynchos*
  23. — *strepera*
  24. — *Penelope* s. *anas fusca*, capite rufo, fronte alba, cauda acuta in lacustribus circa Volgam.
  25. *ANAS Querquedula*
  26. — *Grecca*
- } Hae species rostris dilatatis  
in lacustribus circa Volgam sunt frequentes.
- } Harum specierum innumeri  
greges in paludosis trans Volgam habitant.
27. PELECANUS *Onocrotatus*, corpore albo lutescente in dorso coloris pallide fusci. Rostrum ultra pedem Anglicum longum, inferior mandibula bifurca, cum sacco gulari, in ripa Yeroosiani 18. vidi.
28. ————— *Carbo* cauda aequali, corpore nigro, rostro edentulo, in apice incurvato, pedibus nigris tetradactylis palmatis; in occipite cranium in processum osseum acutum finitur.
29. COLYMBUS *auritus* in Volga & lacubus vicinis obvi.

# GRALLÆ.

30. PLATALEA *Leucorodia* coloris albi, pedibus rostroque, apice rotundato, dilatato, rubris, ad Volgam frequens.
31. ARDEA *Grus* ad Volgam.
32. ————— *Cinerea major* ibidem.



33. ——— *Ibis nigra*, rostro arcuato, pedibus & rostro luteo-rubicundis, corpore nigro, viridescente, magnitudine corvi cornicis, at elatior ob pedes collumque elongatum, longitudo ultra 3 pedes Angl. a rostro ad pedes; gregatim in paludosis Volgae degit, ibique ranis, lacertis & serpentibus victitat.
34. *SCOLOPAX ruficollis* in sylvosis ad Volgam, *the Wood-cock*.
35. ——— *Phaeopus* colore fusco rufescente, pectore griseo. *The Wimbrel*.
36. ——— *Gallinago*. *The Snipe or Snite*.
37. ——— *minima*. Priori similis, sed multo minor, plumulis crinibus similibus. *The Jack-Snipe*.
38. *TRINGA Vanellus*,  
 39. ——— *Glareola*,  
 40. ——— *Hypoleucus*, } ad Volgam admodum  
 frequentes aves.
41. *CHARADRIUS Oedicnemus* in campis desertis trans Volgam.
42. *FULICA atra*, fronte calva coccinea, corpore nigro viridescente, pedibus lobatis viridibus; in paludosis hiemat sub aqua ut hirundines, quod ipse in Borussia expertus sum.
43. *RALLUS Crex* in humidis ad Volgam.
44. *OTIS Tarda* habitat inter sata.
45. — *Tetrax* in campis desertis trans Volgam obvia.

GALLINAE.

46. *PAVO cristatus* a magnatibus alitur, & ova parit quae a gallinis & meleagridibus excluduntur.

47. MELEAGRIS

47. MELEAGRIS *Gallopavus domesticus* ubique alitur.  
 48. PHASIANUS *Gallus*. Copiose alitur haec species.  
 49. TETRAO *Tetrix* in betuletis cis Volgam.  
 50. ——— *Lagopus* hieme albus, exceptis rectricibus, & aliquot pectoris maculis rufis.  
 51. ——— *Coturnix* copiosa, in satis ex utraque parte Volgae.

## PASSERES.

52. COLUMBA *oenas domestica*, in villis alitur.  
 53. ——— *Palumbus* in sylvis frequens.  
 54. ——— *Turtur* major domestica, colore griseo-incarnato, macula laterali colli nigra, in sylvis cis Volgam frequens.  
 55. ALAUDA *arvensis* in utraque Volgae parte, in campis admodum frequens.  
 56. ——— *Yeltoniensis* *rostrum* cylindrico, subulato, recto; *lingua* bifida. *Unguis* posticus rectior; digito multo longior. *Rostrum* ad basin nigrum, crassiusculum, apice albescens. *Corpore* alisque nigris, in capite, dorso, & in summis alis, pennis paucis rufescentibus, sexta remigum margine exteriore alba; duae mediae rectrices rufae. Gregatim trans Volgam in vicinia lacus Yeltoniensis degunt hae Alaudae. *Alaudae Yeltonienses* appellantur. Saporis exquisiti, & admodum sunt pingues mense Augusto, magnitudine Sturni.  
 57. STURNUS *vulgaris*. In pratis trans Volgam innumeri sturnorum greges victitant.  
 58. TURDUS *muscus* in sylvis ad Volgam.  
 59. LOXIA *pyrrhula* in sylvis ad Volgam.

60. *EMBERIZA calandra* } in campis frequens.  
 61. ————— *citrinella* }  
 62. *FRINGILLA coelebs* in sylvis.  
 63. ————— *Carduelis*, } in campis obviae.  
 64. ————— *Linarius*, }  
 65. ————— *Schoeniclus* in arundinetis ad Ylav-  
 lam & trans Volgam.  
 66. ————— *domestica* ubique frequens.  
 67. *MOTACILLA Luscinia*; sylvas cantu replent  
 innumerae.  
 68. ————— *Curruca* in fruticetis ad Volgam.  
 69. ————— *flava* in campis reperitur.  
 70. ————— *alba*, pectore nigro, ad aquas fre-  
 quens.  
 71. *PARUS major* in sylvis.  
 72. ————— *palustris* in salictis & arundinetis prope  
 Volgam.  
 73. *HIRUNDO urbica*, sub tectis nidulatur.  
 74. ————— *riparia*, in deserto ad flumina Yeroo-  
 slan, Targun, Cuba, etc. in foraminibus victi-  
 tat adeo frequens, ut multa millia simul videri  
 possint.

## CLASSIS TERTIA. AMPHIBIA.

### REPTILIA.

1. *TESTUDO lutaria*; minorem ovo exclusam at  
 mortuam reperi, trans Volgam; innumerae hujus  
 generis in lacubus ad Volgam habitare dicuntur.
2. *LACERTA agilis* in campis aestu torrentibus ubi-  
 que obvia, grisea maculis nigris.

3. *RANA*

- |                             |   |   |
|-----------------------------|---|---|
| 3. <i>RANA temporaria</i> , | } | ingens ranarum copia in paludibus ad Volgam; & quod observandum, jam hic clamoribus suis, aërem replent, cum in reliqua septentrionali Russia sint mutae. |
| 4. ——— <i>esculenta</i> ,   |   |   |

## SERPENTES.

5. *COLUBER* magnus ad 2 & 5 pedes Angl. coloris grisei caerulescentis, maculis nigris, sub abdomine lutescens, venenosus esse dicebatur; nonnisi partem exuviarum vidi.
6. ——— coloris lividi, caput triquetrum, corpore latiore macula coloris rubicundi in capite, abdomen crassius; venenatus, in terra & aqua vivit, sibilat; ad Volgam & Yerooslanum invenitur: ad 3 pedes longus.
7. *COLUBER* coloris griseo-fusci, fascia nigra tessellata in dorso, sesquipedem longus, in paludosis vivit.

## NANTES.

*ACIPENSER*. Spiracula lateralia solitaria, linearia.

*Os* sub capite retractile edentulum.

*Cirri* sub rostro ante os, in omnibus quos vidi, quatuor.

*Cauda* sursum recurva.

8. ——— *Sturio* rostro brevi obtuso recto, cirrhi quatuor ante os.

*Caput* compressum, ossa figuris asteriscorum inscripta.

*Corpus* quinquangulare, pyramidatum, dorso fastigiato. *Cutis* scabra cinereo-flavescens. *Caro* rubescens.

*Squamarum* ossæarum series quinque.

Series 1. *Dorsalis* squamis majoribus retrorsum uncinatis 14.

—— 2 & 3 *Laterales* 37.

—— 4 et 5 *Abdominales* inter pinnas pectorales & ventrales 11.

Longitudo 6 ad 16 pedes Angl.

9. ——— *Sbyp*; *rostro* elongato, acuminato, recto. *Cirri* 4 ante os, carne minus sapido, a me non visus.

10. ——— *Ruthenus major* *rostro* elongato acuminato, veluti vagina tecto, paullulum supino. *Cirri* 4 ante os. *Caput* compressum, ossa figuris asteriscorum inscripta. *Corpus* quinquangulare, pyramidatum, dorso fastigiato. *Cutis* scabra flavescens, *Caro* rubra. *Squamarum* ossæarum series 5. Magnitudo a 4 ad 10 pedes.

11. ——— *Ruthenus minor*, *rostro* elongato acuminato recto. *Cirri* 4 ante os. *Caput* compressum 5. *Corpus* quinquangulare. *Cutis* scabriuscula, atrocinerea. *Caro* rubra. *Squamarum* ossæarum series 5. Magnitudo ad 4 pedes.

12. ——— *Ruthenus minor*, *rostro* brevi obtuso, recto. *Cirri* 4 ante os. *Caput* compressum, ossa figuris asteriscorum inscripta. *Corpus* quinquangulare, pyramidatum, dorso fastigiato. *Cutis* scabriuscula, colore atro-cinereo, in abdomine flavescens, circa caudam tessellata. *Caro* alba. *Squamarum* ordines 5 ossæarum, uncis parvis instructæ.

Series 1 *Dorsalis* squamis majoribus retrorsum uncinatis 12, 13, 14 vel 15.

—— 2 & 3 *Laterales* 69 vel 70.

—— 4 & 5 *Abdominales* 10 vel 11 inter Pect. & Vent. pinnas.

Longitudo raro 3 vel 4 pedes excedit.

Pinna dorsalis prope caudam officulis 46. Pin.

Pect. 36. cum prima rigida. Pin. Ventr. 24.

Anal. 27. Caud. 150.

13. ——— *Beluga* f. *Albula*. Rostro brevi obtuso recto. *Cirri* 4 ante os. *Caput* triquetrum.

*Corpus* teres conicum. *Cutis* laevis, in dorso nigricans, sub abdomine alba, circa caudam tessellata.

*Caro* alba. *Squamarum* series 5 depressae. *Longitudo* a 4 ped. Angl. ad 16. Tres ejus varietates dicuntur, quas non vidi.

*Observatio*. Omnes hi pisces ex membrana interiori (Angl. *sound* dicta) dant Ichthyocollam, at *Beluga* praebet optimam. Sic & garum ex omnium acipenserum ovis fieri potest, ex Rutheno minore optimum. Verno tempore raro hi pisces capiuntur, aestate pauci, ut plurimum tamen Rutheni minores. Autumno jam frequentiores capturae; hieme maximae piscaturae sub glacie instituuntur. Hi enim pisces in profundissima loca fluminum Volgae & Yaiki gregatim coeunt, & omnem locum ab imo fundi fluminis ad glaciem usque replent. Hibarnis copiis acipenserum inventis, die post Festum Christi nati, omnes accolae securibus & uncis conto longissimo impactis, instructi ad hunc locum contendunt; aperta glacie quilibet uncum demittit, & ope ejusdem pisces in glaciem extrahit; si majoris molis piscem est nactus, advocat.

advocat vicinos ut opem ferant: junctis itaque viribus piscem extrahunt, & in commune vendunt. Sic quotannis multa millia Acipenserum capiuntur, & excepto Rutheno majore, omnes Caspii littoribusque vadosis ova deponere videntur; hujus solius pisciculi minuti duarum unciarum in fluminibus capiuntur, reliqui nunquam 8 unciiis minores.

## CLASSIS QUARTA. PISCES.

### THORACICI.

1. PERCA *fluviatilis*, pinnis dorsalibus distinctis, secunda radiis sedecim, lineis utrimque sex nigris, pinnis ventralibus ruberrimis. Perch.
2. ——— *Lucio-perca*, pinnis dorsalibus distinctis, secunda radiis 25.  
Pinna Dorsi I. 14. II. 25. P. P. 15. P. V. 6. P. A. 15. C. 22. cauda sinuata.
3. ——— *Lucio-perca minor*, pinnis dorsalibus distinctis, secunda radiis 22.

### ABDOMINALES.

4. ESOX *Lucius* Membr. Branchiof.
- P. D. 22. P. P. 14. P. V. 11. P. A. 18. C. 25.
5. CLUPEA Membr. Branchiof. VIII.
- P. D. 15. P. P. 14. P. V. 9. P. A. 19. C. 25.
- Latitudo ad Longitudinem  $\frac{1}{4}$ . Venter cultratus, abdominis carina spinis acutissimis praedita, squamae argenteae, saepe ad duos pedes longus.

6. CYPRINUS *Carassius*, cauda integra, squamis aureis nigricantibus, linea laterali recta.  
P. D. 19. P. P. 15. P. V. 9. P. A. 8. C. 24.
7. ——— *Tinca*, cauda integra, corpore mucoso fusco.  
P. D. 11. P. P. 17. P. V. 11. P. A. 10. C. 25.
8. CYPRINUS, an *Cephalus*? Dorso capiteque crasso, pinnis ventrali & anali rubicundis, squamis magnis; longus  $\frac{1}{2}$  ped. usque ad 2 ped. Parisinos; latus 4 uncias, circa pinnae ventrales, cauda integra.
9. ——— *Idus*. Cauda bifida.  
P. D. 11. P. P. 16. P. V. 9. P. A. 12. C. 23.
10. ——— *Jeses* pinnae ani & ventrales rubrae pectorales, fuscae, cauda bifida.  
P. D. 11. P. P. 15. P. V. 9. P. A. 12. C. 22.
11. ——— an *Aspius*? capite acuto, squamis magnis, longus 2 ped. Parisin. lat uncias  $5\frac{1}{2}$  circa pinnae ventrales.  
P. D. 11. P. P. 18. P. V. 9. P. A. 16. C. 23.
12. ——— *Alburnus*.  
P. D. 10. P. P. 16. P. V. 8. P. A. 18. C. 22.
13. ——— *Brama minor* pinnis omnibus nigricantibus, nonnisi magnitudine a *Brama* differt, nec tamen idem, aetate diversus.  
P. D. 12. P. P. 15. P. V. 9. P. A. 24. C. 27.
14. ——— *Brama major*.  
P. D. 12. P. P. 15. V. 9. A. 27. C. 28.
15. ——— *cultratus*, ventre acuto cultrato, dorso recto, squamis argenteis minoribus, longus usque ad duos pedes Parisinos, lat. uncias  $4\frac{1}{2}$ .  
P. D. 10. P. P. 17. P. V. 8. P. A.  $\frac{27}{11}$ . C. 26.



16. CYPRINUS, an *Ballerus*? admodum latus & tenuis, argenteis squamis.

P. D. 11. P. P. 16. P. V. 9. P. A. 29-30. C. 25.

Hic subsisto; & Specimen Historiae Naturalis Volgenfis finio, sperans fore ut eo & plures excitentur ad his perfectiora edenda.

Dabam Londini, die xxviii

Febr. anni 1767.

Joannes Reinholdus Forster.

XXXIV. *De Problemate quodam Algebraico, deque evolutione mechanicae cujusdam Curvæ inter infinitas hypermechanicas, quæ determinatæ æquationi satisfaciunt. Auctore Pio Fantoni, Mathematico Bononienfi. Communicated by Sir Horace Mann, His Majesty's Envoy at Florence.*

Read June 25, 1767. **Q**UI in computationibus analyticis versari solet, animum non modo in ea præsidia solet intendere, quibus problemata deducuntur ad æquationes, sed maximam ubique exoptat concinnitatem, atque elegantiam, ut universi operis apparatus, constructio, utilitasque commendentur. Quantum vero elaborationis, ac studii plerumque ad hæc singula requiratur, ii probe intelligunt excellentissimi viri, qui se jamdudum algebrae dediderunt. Veruntamen fateri ultro debent, non mediocrem aliquando utilitatem obtineri posse ex hujusce potius, quam illius methodi applicatione. Fit enim non raro, ut cum quæstionem aliquam subtili licet ingenio versatus fueris, alia tandem methodus meliori auspicio suscepta, illico tibi elargiatur clariorem uberioremque ejusdem quæstionis solutionem, ex qua multa præterea obtineas quæ admireris. Id vero mihi

in sublimi problemate quodam algebraico, an bene feliciterque contigerit, vestro iudicio, Academici sapientissimi, quod maximi facio, decernendum relinquo. Si interea, ut exoro, hanc meam elucubrationem summa humanitate vestra excipietis, eâ deinceps utar in aliis non contemnendis rebus tum physicis, tum mechanicis, ut vestra sapientia duce, facilius ad quasdam naturæ adhuc reconditas leges pervenire possim.

Nunc porro velim intelligatis, me in hoc argumento analytico, de quo loquor, tria maxime præstitisse. Primum enim curvam quandam exploravimus suis coordinatis ad axem, & licet lectissimis præfidiis usi fuerimus in separatione indeterminatarum, licet investigationem nostram satis ultro promotam conspexerimus, in æquationem tandem, ut dicunt, hypermechanicam irrumpere opus fuit, in eam videlicet, quæ exposcit mechanicam quadraturam curvarum exigentium primo mechanicam circuli quadraturam. Ex hac autem fere inextricabili constructione non ea certe consecutaria, quæ in votis erant, erui elegantissime potuissent. Quare difficultate rei veluti commoti, satis opportune curvam nostram ab axe ad focus deduximus, atque hoc modo universum illud opus, cujus dilucide enodandi spem omnem antea demisimus, eò tandem feliciter perduximus, ut ipsum recte esset, nobisque plene satisfaceret. At vero in hac secunda problematis mei parte dum contendendo, dum illam constanti animo defugio hypermechanicam constructionem, atque ad pure mechanicam propero, invenio tandem in infinita curvarum hypermechanicarum familia, quæ peculiari cuidam

cuidam æquationi satisfaciunt, unam præterea curvam opportune abscondi, quæ a simplici quadratura circuli dependeat, quæque nobis constructionem plenam aptioremque impertiat, sitque semper in potestate, dummodo  $y$  dentur per  $x$ , quanquam separari indeterminatæ nullo pacto ab invicem possint. Qui rem hanc altius perscrutari curabunt, non difficili labore intelligent in æquationibus naturæ hujus semper includi curvam similem nostræ simili modo detegendam. Cujus sane methodi cognitio an utilitati & commodo Analystis futura sit in hujusmodi operosissimis quæstionibus solvendis, non est cur diuturniori oratione vobis exponam. Properamus itaque ad rem nostram.

## PROBLEMA.

### TAB. XIV. FIGURA PRIMA.

Invenire curvam  $IMm$  ea proprietate donatam, ut ex dato puncto  $A$  ductâ in tangentem  $MT$  perpendiculari  $AG$ , intercepta  $MG$  sit semper æqualis constanti  $a$ .

Ex puncto contactus  $M$  in axem  $AP$  duc perpendicularem  $MP$ , eique parallelam infinite proximam  $mp$ , tum excita ex puncto  $M$  rectam  $Ms$  parallelam  $Pp$ , dicque  $AP = x$ ,  $Pp = Ms = dx$ ,  $PM = y$ ,  $ms = dy$ , &  $MG = a$ . Erit  $Mm = \sqrt{dx^2 + dy^2}$ .  $PT = \frac{ydx}{dy}$ .  $TA = \frac{ydx - xdy}{dy}$ ,  $TM = \frac{y}{dy} \sqrt{dx^2 + dy^2}$ , ideoque  $GT = \frac{y\sqrt{dx^2 + dy^2}}{dy} - a$ .











At propter similia triangula  $Msm$ ,  $TGA$ , erit  
 $Mm : Ms :: TA : TG$ . seu  $\sqrt{dx^2 + dy^2} : dx :: \frac{ydx - xdy}{dy}$  :  
 $\frac{y\sqrt{dx^2 + dy^2}}{dy} - a$ ; unde facta extremorum mediorum-  
 que multiplicatione, erit  $a\sqrt{dx^2 + dy^2} = ydy + xdx$ , seu  
 $x = -\frac{ydy}{dx} + \frac{a\sqrt{dx^2 + dy^2}}{dx}$ .

Fac autem pro separatione indeterminatarum  $x =$   
 $\int \frac{tdy}{a}$ , & consequenter  $dx = \frac{tdy}{a}$ , tumque hos valores  
 pro  $x$ , &  $dx$  substitue in data æquatione. Habebis  
 $\int \frac{tdy}{a} = -\frac{ay}{t} + \frac{a\sqrt{t^2 + a^2}}{t}$ , & facta differentiatione, erit  
 $\frac{tdy}{a} + \frac{ady}{t} - \frac{aydt}{t^2} = \frac{-a^2dt}{t^2 \cdot a^2 + t^2 \cdot \frac{1}{2}}$  seu  $\frac{dy}{y} - \frac{a^2dt}{t \cdot a^2 + t^2} = \frac{-a^2dt}{ty \cdot a^2 + t^2 \cdot \frac{1}{2}}$ .

Cum porro ex nota Bernoullii methodo sit  $\frac{a^2dt}{t \cdot a^2 + t^2}$   
 $= \frac{dt}{t} - \frac{tdt}{t^2 + a^2}$ , pone claritatis gratia hæc logarith-  
 micas quantitates  $= \frac{dn}{n}$ . Hinc habebis  $\frac{dy}{y} - \frac{dn}{n} =$   
 $\frac{-a^2dt}{ty \cdot a^2 + t^2 \cdot \frac{1}{2}}$ . Positis autem  $\frac{dy}{y} - \frac{dn}{n} = \frac{dp}{p}$ , atque ideo  $\frac{p}{n} =$   
 $\frac{p}{a}$ , seu  $y = \frac{np}{a}$ , & facta harum quantitatum substituri-  
 one, obtinebis  $\frac{dp}{p} = \frac{-a^2dt}{npt \cdot a^2 + t^2 \cdot \frac{1}{2}}$ , seu  $\frac{dp}{p} = \frac{-a^2dt}{t^2 \times t^2 + a^2}$ ,  
 posito videlicet jam primum  $n = \frac{at}{\sqrt{t^2 + a^2}}$  ex quo se-  
 quitur  $\frac{dp}{p} = \frac{a^2dt}{t^2} + \frac{a^2dt}{a^2 + t^2}$ .

Sed quia constituimus superius  $x = \int \frac{tdy}{a}$ , &  $y = \frac{zp}{a}$ ; si in hisce valoribus coordinatarum  $x$  &  $y$  substituas æquivalentes valores expressos dumtaxat per  $t$ , &  $dt$ , habebis demum

$$x = \int \frac{at dt}{t^2 + a^2} \times \int \frac{a^2 dt}{t^2 + a^2}$$

$$\& y = \frac{t}{a^2 + t^2} \times \frac{a^2}{t} + \int \frac{a^2 dt}{t^2 + a^2}.$$

Qui vero hujusmodi formulas ad constructionem revocare statuerit, intelliget ille quidem infinitis dumtaxat curvis problema nostrum plane exhaustiri posse.

\* At quis non dixerit  $\int \frac{at dt}{t^2 + a^2} \times \int \frac{a^2 dt}{t^2 + a^2}$ , exigere

constanti lege in quolibet casu quadraturam mechanicæ cujusdam curvæ, quæ ipsa primum a mechanica circuli quadratura dependeat? Primo certe hujusce formulæ ad spectu nemo non judicaverit problema nostrum hypermechanicum fore; maxime vero cum nulla directâ methodo, quantum mihi constat, compertum sit, hujusmodi formulas revocari posse ad alias, quæ a sola circuli quadratura dependeant. Quapropter in illa ego opinione adhuc essem, ut solæ hypermechanicæ curvæ aptæ forent satisfaciendo problemati nostro, si quæsitam curvam ab axe non traduxissem ad focum; ex quo illico certior factus sum, quæstiones hujusmodi,

\* \* Fluens  $\int \frac{at dt}{t^2 + a^2} \times \int \frac{a^2 dt}{t^2 + a^2}$  a sola circuli quadraturâ pendet & hæc est  $\frac{at}{a^2 + t^2} - \frac{a}{a^2 + t^2} \times \int \frac{a^2 dt}{a^2 + t^2}$  (arc circuli, cujus radius est  $a$  & tangent  $c$ .) E. W.

quas

quas ab initio dixeris implicatissimas, seu pene inextricabiles, sola tandem circuli quadratura expediri feliciter posse. Quo autem modo id factum a nobis fuerit brevi expono.

FIGURA SECUNDA.

Referatur \* quæsitæ curva  $IMm$  ad focum  $A$ , ex quo ductis duabus ordinatis  $AM$ ,  $Am$  minimum angulum continentibus, centro  $A$ , radio  $AM$  describatur infinitesimus arcus  $Md$ , tum vocetur  $AM = z$ ,  $Md = dx$ ,  $Mm = ds$ ; ductaque tangente  $MG$ , atque in ipsam ex puncto  $A$  perpendiculari  $AG$ , fiat intercepta  $MG = a$ , unde perpendicularis  $AG$  erit  $= \sqrt{z^2 - a^2}$ .

Propter similia triacula  $AMG$ ,  $Mdm$  erit  $Mm : md :: MA : MG$ ; seu  $ds : dz :: z : a$ . ergo  $zdz = ads$ .

& integrando  $Aa + as = \frac{z^2}{2}$ . Constat itaque curvam quæsitam esse rectificabilem, estque  $A$  quantitas addenda, si opus fuerit, æquationi complendæ, quam  $A$  deinceps determinabimus.

Erigatur interea æquatio differentialis ad quadratum, & orietur  $z^2 dz^2 = a^2 dz^2 = a^2 dz^2 + a^2 dx^2$ , ob triangulum  $Mdm$  infinitesimum rectangulum in  $d$ . Hanc ergo habebis  $\overline{z^2 - a^2} . dz^2 = a^2 dx^2$ ; sive  $dz \sqrt{z^2 - a^2} = a dx$ , quæ est æquatio quæsitæ curvæ relatæ hoc modo ad focum  $A$ .

Multiplicetur hæc ultima æquatio per  $\frac{z}{2a}$ ; fiet  $\frac{zdz}{2a} \sqrt{z^2 - aa} = \frac{zdx}{2}$ . Integretur; habebis  $aB +$

\* Hujus curvæ arcum, longitudinem, evolutam & radii curvaturæ jamdudum invenit Simpson; consulas enim pagin. 151 & 163 in tractatu suo de fluxionibus. E. W.

$\frac{zx - aa\sqrt{zx - aa}}{2.3.a} = \int \frac{zdx}{2}$ . Cum autem  $\frac{zdx}{2}$  sit elementum areæ, patens est curvam esse quadrabilem. B est quantitas addenda, si opus ea fuerit, in integratione.

Ut vero redigatur æquatio superius inventa ad arcum radii constantis, abscinde  $AE = a$ , & describe arcum minimum Ee, quem voca  $= du$ . Habebis  $z : a :: dx : du$ . ergo  $dx = \frac{zdu}{a}$ ; quo valore substituto in superiori æquatione  $dz\sqrt{z^2 - a^2} = a dx$ , hæc mutabitur in istam  $\frac{dz}{z}\sqrt{z^2 - a^2} = du$ , in qua insunt variables separatæ.

Ut primum membrum ad formulas magis cognitæ, reducatur, ita æquationem dispono  $\frac{zdz\sqrt{z^2 - a^2}}{zz} = du$ ; tum constituo  $AG = \sqrt{z^2 - a^2} = t$ , factaque substitutione, orietur  $\frac{t^2 dt}{t^2 + a^2} = dt - \frac{a^2 dt}{t^2 + a^2} = du$ . Formula  $\frac{a^2 dt}{t^2 + a^2}$  ut constat, est elementum arcus circularis, cujus radius  $= a$ , tangens  $= t$ .

Ultima igitur hæc æquatio ad constructionem perducit, quæ circuli quadraturam supponit. Centro itaque A, radio  $AI = a$ , describatur circulus ILP, cui sit tangens indefinita IK. Sumatur in hac tangente quælibet  $IH = t$ , & agatur secans  $AH = z$ ; sume præterea differentiam inter tangentem IH, & ejus arcum IL, quæ erit  $= u$ ; tandem accipe arcum IL huic differentiæ æqualem, & per punctum E duc  $AM = AH$ , punctum M erit in curva quæsitæ.

Ex hac constructione facile colligitur curvam nostram incipere in puncto I, tum ad modum spiralis semper recedere a circulo, & infinitis circumvolutionibus illum ambire. In puncto I curva tangitur a radio IA. Nullam addidi in mea constructione constantem, propterea quod constantis additio curvam non mutat. Nam IE vel sit æqualis  $u$ , vel  $u + b$ , vel tandem  $u - b$ , eadem prorsus curva enascitur.

Nunc vero sunt determinandæ constantes A & B, quæ additæ sunt in integratione, dum curvæ rectificationem, & quadraturam invenimus. Quoniam posito  $s = 0$ , fit  $z = a$ , æquatio  $\frac{z^2}{2} - Aa = as$ , data hac hypothefi, in istam mutabitur  $\frac{aa}{2} - Aa = 0$ , unde  $A = \frac{a}{2}$ ; quapropter æquatio completa erit  $\frac{zz - aa}{2a} = s$ . Atqui  $zz - aa = H$ . ergo  $\frac{H}{2a} = s$ .

Quod spectat ad quadraturam, jam constat fore aream  $AIM = 0$ , cum fit  $z = \text{radio}$ , seu  $= a$ ; ergo æquatio  $aB + \frac{zz - aa\sqrt{zz - aa}}{2.3.a} = \int \frac{zdx}{2}$ , evadit in hac hypothefi in istam  $aB = 0$ : ergo æquatio completa est  $\frac{zz - aa\sqrt{zz - aa}}{2.3.a} = \frac{t^3}{2.3.a} = \int \frac{zdx}{2}$ . Sed  $\frac{t^2}{2a} = s$ ; ergo  $\frac{t^3}{3} = \int \frac{zdx}{2}$ ; ideoque spatium IAM est tertia pars rectanguli ex AG; seu IH, & ex curva IM.

Radium osculi hac ratione describemus. Ducatur radius AR perpendicularis rectæ AG, & jungatur RM. Quoniam GM, AR æquales sunt, & parallelæ, GA, MR pariter æquales erunt, & parallelæ. Ergo MR

MR perpendicularis radio AR tanget circulum, & perpendiculariter occurret curvæ  $Mm$ . Eodem prorsus modo ducto radio  $Ar$  normali rectæ  $Ag$ , linea  $mr$  erit tangens circuli, & normalis curvæ  $Mm$ . Igitur curva  $IMm$  ea est quæ nascitur ex evolutione circuli, & recta  $MR = AG$  æquabit arcum circulearem  $IR$ .

Quoniam vero  $RM = AG = IH$ , &  $IH$  ex constructione æquat duos arcus circulares  $IE$ ,  $IL$ , arcus  $IR$  æquabit duos arcus  $IE$ ,  $IL$ , & dempto communi  $IL$ , remanebit arcus  $IE = LR$ .

Infinitefimus sector  $RMm$ , qui est elementum areæ  $REIM$  æqualis est  $\frac{tds}{2}$ . Sed  $ds = \frac{zdz}{a} = \frac{tdt}{a}$ . ergo

$$RMm = \frac{t^2 dt}{2a}. \text{ Et integrando area } REIM = \frac{t^3}{2 \cdot 3 \cdot a}.$$

Sed etiam area  $IAM$  supra inventa est æqualis  $\frac{t^3}{2 \cdot 3 \cdot a}$ ,

ergo area  $REIM = IAM$ . Et ablato spatio communi  $IEM$ , remanet sector  $IAE = MER$ . Addito autem sectore  $EAR$  fit sector  $IAR =$  triangulo  $AMR$ , quod apprime cum veritate consentit; nam cum arcus  $IR = RM$ , constat sectorem  $IAR$  æquare triangulum  $ARM$ .

Curva transiens per omnia puncta  $G$  g. erit basis, ex qua gignitur tractoria  $IMm$ . Quænam sit hæc curva breviter videamus. Quoniam  $GAN$ , &  $MRm$  sunt sectores similes, &  $AG = RM$ , erit  $Gn = Mm = ds$ . Ergo æquatio  $ads = tdt$ , erit æquatio curvæ quæsitæ, existente ordinata  $AG = t$ ,  $Gn = ds$ . Ut autem æquatio reducatur ad arcum radii constantis, vocetur  $Tf = dw$ ; erit  $t : a :: ds : dw$ . Ergo  $ads = tdw$ .

Ergo

Ergo  $tdt=tdw$ , five  $dw=dt$ , five tandem  $Tt=gn$ , quæ est æquatio spiralis Archimedææ, cujus constructio ita peragitur.

Age radium AP, perpendicularem radio AI, & sumatur arcus PQ æqualis radio; tum polo A, describatur spiralis Archimedæa transiens per punctum Q; hæc ipsa erit basis, ex qua describitur tractoria IM prædita tangente constanti  $GM=a$ .

Interea hæc habe: spiralis Archimedæa est ea curva, a qua tamquam basi nostra generatur tractoria IMm. Nunc superest animadvertere, quod si in illa formula, quam vir clariss. Vincentius Riccatus methodo motus tractorii construxit in suo commentario de usu hujus motus in æquationum differentialium constructione (ubi hanc methodum illustravit penitusque absolvit) si, inquam, in illa formula supponas  $x$  &  $y$  esse coordinatas spiralis Archimedææ, &  $y$  datas esse per  $x$ , quamquam indeterminatæ separari omnino nequeant, suscipiet dicta formula ex infinitis, quarum est capax, unam quoque constructionem dependenter a nostra curva. Ea ex quatuor Riccatianis ibidem expositis formulis, quæ hypothese nostræ convenit prima est, nimirum,

$$\frac{abdz}{\sqrt{bb+qq}} + qdx = bdy.$$

Facta ergo, ut dixi, suppositione, ejus  $x$  &  $y$  esse coordinatas spiralis Archimedææ, si infinita puncta N construendæ curvæ tuto invenire cupias, exigit illa methodus, ut descripta tractoria IMm ope sili, seu tangentis constantis  $GM=a$ , facto jam motu aG versus Q, tumque sumpta in axe quacumque constanti  $OS=b$ , semper ad eandem partem, si per punctum S. ducas

ducas parallelam tangenti GM, donec occurrat ordinatæ  $OG=y$  in puncto N, hocce punctum, ut ibi demonstratur, est semper in quæsitâ curvâ. Atqui vidimus supra rectam RA parallelam tangenti GM hujus nostræ tractoriæ.  $IM^m$  fore perpendicularem radio AG spiralis Archimedææ AQG.

## FIGURA TERTIA.

Ergo ut habeas infinita puncta N.N. construendæ curvæ, sufficit quod sumas semper in axe constantes PG,  $PG=b$  ad eandem plagam, tumque a punctis G, G ducas in radios spiralis AM, AM productos, si oporteat, normales GN, GN, donec occurrant ordinatis PM, PM in N.N. Hoc modo obtinebis per infinita puncta curvam hac methodo describendam. Invenies itaque hujusmodi curvæ ramum genitum a spiralis arcu AMS esse ANT; ab altero vero spiralis arcu SMO esse ANQ; a tertio OMR esse ANV; a quarto RK esse AY; a quinto denique KZ esse AW, & sic in infinitum asymptoticos omnes; ex quo propterea vides integram curvam, quæ nostræ formulæ constructionem suppeditat in hac videlicet peculiari tractoria abdita ramis numero infinitis gaudere, ac eorum quemlibet votis satisfacere recte posse.

Sed quia ad obtinendam dictæ formulæ constructionem opus maxime est ut abscissæ  $x$  sint in axe, earum vero ordinatæ  $y$  sint omnes inter se parallelæ (nostræ autem  $y$  hic sunt ad focus) ac propterea oportet ut eadem  $y$  datæ sint per  $x$ , vel postea separari indeterminatæ possint, vel non, nunc ergo ut hisce conditionibus



onibus compleam, satis mihi erit invenire æquationem spiralis Archimedææ relatæ ad axem, quod sic assequor.

FIGURA QUARTA.

Sit spiralis Archimedæa  $AmM$ , ejus axis  $FAF$ , abscissa  $AP = x$ , ordinata  $PM$  ad angulum rectum  $= y$ , eique infinite proxima  $pm$ . Ducta  $mo$  parallela ad axem, erit,  $mo = dx$ ,  $oM = dy$ . Sit propterea  $AM$  radius spiralis  $= t$ , cum quo  $Am$  faciat angulum infinitesimum  $MAm$ , & centro  $A$ , radio  $AM$ , descripto circuli arcu  $mr$ , erit  $Mr = dt$ . Voca arcum  $mr = ds$ , & eodem centro  $A$ , radio quovis constanti  $= a$ , describe circumulum  $Fcb$ , & voca ejus arcum infinitesimum  $cb = du$ .

Ex hac præparatione erit primò  $\overline{AM}^2 = \overline{AP}^2 + \overline{PM}^2$ , seu  $t^2 = x^2 + y^2$ , &  $t = \sqrt{x^2 + y^2}$ ; unde  $dt = \frac{x dx + y dy}{\sqrt{x^2 + y^2}}$ . Præterea habebis  $\overline{Mr}^2 + \overline{rm}^2 = \overline{Mm}^2$  seu  $dt^2 + ds^2 = dx^2 + dy^2$ . Sed ex similitudine Sectorum  $Ac b$ ,  $Amr$ , est  $Ac : cb :: Am : mr$ ; seu  $a : dt :: t : ds$ . & ex æquatione spiralis Archimedææ ad focum habes  $cb = Mr$ , seu  $du = dt$ ; unde erit  $ds = \frac{t dt}{a}$ . Ergo factis opportune substitutionibus in altera superiori æquatione, obtinebis  $dt^2 + \frac{t^2 dt^2}{a^2} = dx^2 + dy^2$ , seu tandem

$$\overline{a^2 + x^2 + y^2} \times \frac{x dx + y dy}{x^2 + y^2} = a^2 \times \overline{dx^2 + dy^2}. \text{ vel potius}$$

$$dy^2 + 2\overline{dx}dy \times \frac{a^2xy + x^3y + xy^3}{x^2y^2 + y^4 - a^2x^2} + \overline{dx}^2 \times \frac{x^4 + x^2y^2 - a^2y^2}{x^2y^2 + y^4 - a^2x^2} = 0$$

unde completo quadrato, & facta radicis extractione, erit

$$\frac{dy}{dx} = \frac{-xy \times \overline{a^2 + x^2 + y^2} \pm a \times \overline{x^2 + y^2}^{\frac{1}{2}}}{x^2y^2 + y^4 - a^2x^2}.$$

Ecce itaque spiralis Archimedea æquationem relatæ ad axem, ut optabamus, in qua  $y$  datur per  $x$ .

\* Quamquam vero in hujusmodi æquatione, indeterminatæ separari nullo artificio possint, vides tamen præfidiis pure mechanicis ad constructionem nos feliciter pervenisse, quod, attento illius summatoriae adspectu, quam initio obtinuimus, cum curvam nostram ad axem referre placuit, impossibile videbatur.

Scio ego quidem constructionem hancce, quæ a sola circuli quadratura dependet, non penitus exhaurire supradictam Riccatianam formulam, quippe quæ construi etiam potest, quacumque alia proposita tractoria, cujus basis sit dicta spiralis Archimæda, ejusque tangens recta quævis linea constans; sed inter infinitas hæc constructiones nostra quidem maximum locum habet, ut quæ cæteris simplicior, nec minus vera.

Porro antequam finem facio, unum addam. Læodædus Mathematicus in capite secundo sui commentarii ostendit, quod ubi in constructione suæ formulæ tractoriam circuli adhibeat, tunc in infinitas occurrunt transcendentes curvas, quæ simul exhaurire valent

\* Ad hunc modum indeterminatæ separari possunt; substituatur pro  $x = \frac{z}{a} \times \int \frac{adz}{\sqrt{a^2 - z^2}}$ , & pro  $y = \frac{\sqrt{a^2 - z^2}}{a} \times \int \frac{adz}{\sqrt{a^2 - z^2}}$ , & sic. E. W.

propositam.

propositam formulam. Sed (quod ei merito quidem in pretio est) in infinita harum curvarum familia unam insuper latentem detegit algebraicam quarti gradus, quæ commodè ejusdem formulæ constructionem suppeditat, rectèque perficit. Nos in re fortasse difficiliori non dissimile exemplum hic attulimus. Vidimus enim problema nostrum, quod per tractoriam spiralis Archimedæ generatim construitur, exposcere & ipsum ad sui constructionem curvas numero infinitas, sed quod molestius videtur, magisque operosum, hujusmodi esse hæc curvas, ut nisi hypermechanico labore possimus assequi. Veruntamen in infinito harum agmine facile & nobis fuit ostendere unam præterea curvam abscondi, quam illico assequaris dependenter a sola quadratura circuli, ideoque attenta rei difficultate, multo simpliciori modo, quàm initio sperare licuisset. Noverim certe curvam hanc nostram non plene exhaustire datam formulam, sed infici nequit, ejusdem exhibere nullo fere negotio rectissimam, maximeque simplicem constructionem, quod satis est, aliisque planiorem viam ostendere, qua facilius enodare possint hujusce generis quæstiones inextricabiles primo intuitu, nec vero labore vacuas. Hoc itaque inventum credidimus non contemnendum fore, præsertim cum aliæ methodi usque adeo notæ, quovis versatæ studio, minime quantum nobis constat, ad id commodum perducere valeant.

Romæ prid. Non. Aprilis,

1766.

Pius Fantonus,

Philosophus & Mathematicus Bononiensis.

XXXV. *A Memoir concerning the most advantageous Construction of Water-Wheels, etc. by Mr. Mallet of Geneva. Communicated by M. Maty, M. D. Sec. R. S. Translated from the French, by J. Bevis, M. D. R. S. S. Read March 26, 1767.*

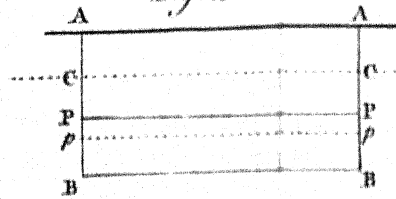
§I. **T**HE stream of rivers is of such importance in moving machines of all kinds, that any attempt towards perfecting this part of mechanics may be considered as of very great utility.

The first wheel on which the stream acts is one of the most essential members of the machine, and it is easy to discern that the greater or less effect thereof must depend, in a great measure, on the manner of constructing this wheel, and on the dimensions given to it. I shall not at present inquire whether wheels of different constructions from those which have been long in use might be advantageously substituted in their stead; but confine myself, in this essay, to an examination of the most common ones, and to discover the means by which they may be made to produce the greatest possible effects. Their construction is very simple; they consist of several planes inserted into the same axle placed horizontally above the surface of the water, and in a position perpendicular to the stream. These planes, called  
float-

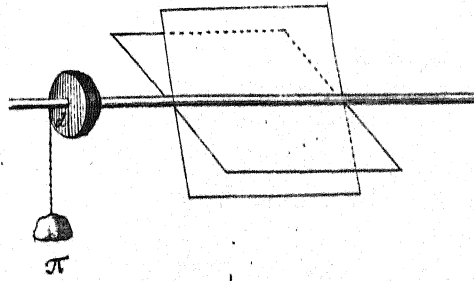




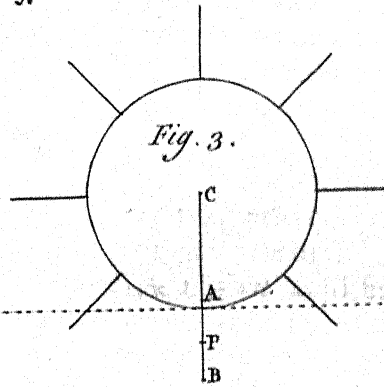
*Fig. 2.*



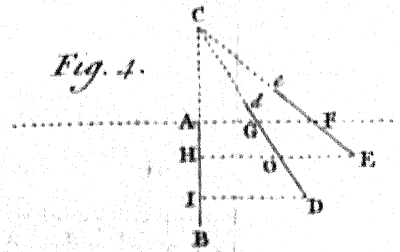
*Fig. 1.*



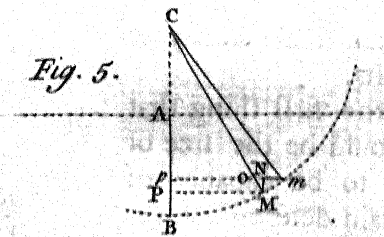
*Fig. 3.*



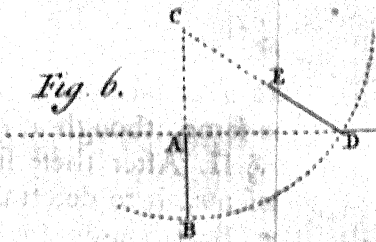
*Fig. 4.*



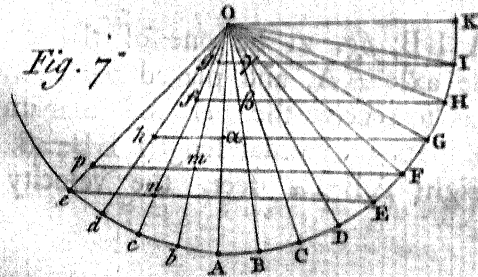
*Fig. 5.*



*Fig. 6.*



*Fig. 7.*







*float-boards*, by yielding to the action of the stream, cause the axle on which they are fixed to turn round, by means of several wheels, which take into each other, and give motion to the part destined to produce some purposed effect, as the mill-stone in a corn-mill.

The size of the float-boards, the velocity with which the wheel is to turn, and the number of the float-boards to produce the greatest possible effect, are three main things I propose to examine in the following inquiry.

In the first place I will suppose the total resistance which this wheel has to encounter, on the part of the machine, and which hinders it from moving so swift as the stream, to be expressed by a weight  $\pi$ , suspended to the extremity of a cord fixed to the circumference of a wheel whose radius is  $d$ , and which has the same axle as the float-board wheel, so that the effect of the stream is to raise the said weight  $\pi$ , as expressed in TAB. XV. *fig.* 1. I will likewise suppose; that the stream, by its velocity, moves through  $v$  feet in one second of time, and that this velocity is the same, though at different depths.

§ II. After these suppositions, the first thing that presents, is to determine what should be the size of the float-boards for the stream to be capable of raising the weight  $\pi$  with a certain determinate velocity.

Let AA BB; *fig.* 2. be one of the float-boards let into the axle AA, and placed vertically in the water, so as to receive the perpendicular impulse of the stream. Its horizontal length BB= $\beta$  feet, its vertical height AB= $a$  feet, the velocity of the wheel

wheel at the point B, such that it shall run through  $z$  feet in a second;  $n$  pounds the weight of a cubic foot of water; and I will suppose the impulse of the stream on a plane perpendicular to it is (as Dr. *Daniel Bernoulli* has stated in his *Hydrodynamica*) equal to the weight of a prism of water, whose base is the plane, and its altitude the generating height of the velocity with which the plane is impelled. This being supposed; let  $AP = x$ ,  $Pp$ , its differential,  $= dx$ , which will give the velocity of the float-board at the point  $p = \frac{x}{a}z$ , and the relative velocity of the stream with which the plane is impelled at the same point  $= v - \frac{x}{a}z$  whose generating height is  $\frac{(v - \frac{x}{a}z)^2}{60}$

feet, whence we have the weight of the parallelo-piped of that height, and of the base  $PP\ p p$  equal to  $\frac{n\beta dx}{60} (-v + \frac{x}{a}z)^2$  pounds, which weight multiplied by the length  $AP$  ( $x$ ) of the lever which tends to turn the plane, will give  $\frac{n\beta x dx}{60} (v - \frac{x}{a}z)^2$  for the total effect of the stream on the little rectangle  $PP\ p p$ , whose integral is  $\frac{n\beta}{60} \int (v v x dx - \frac{2 v x}{a} x x dx + \frac{z z}{a a} x x dx) =$   
 $\frac{n\beta}{60} (\frac{1}{2} v v x x - \frac{2}{3} v x \frac{x^3}{a} + \frac{1}{4} z z \frac{x^4}{a a} - \frac{1}{2} v v f f + \frac{2}{3} v x \cdot$   
 $\frac{f^3}{a} - \frac{1}{4} \frac{z z f^4}{a a})$  (putting  $AC = f$  for the distance between the axle and the surface of the water when the float-board has only its part  $CB$  plunged in the water) which (putting  $x = a$ ) will become  
 $\frac{n\beta}{60} (\frac{1}{2} v v - \frac{2}{3} v z + \frac{1}{4} z z \cdot a a - \frac{1}{2} v v f f + \frac{2}{3} \frac{v z f^3}{a a}$

—  $\frac{1}{4} \frac{zzf^2}{aa}$ ) and will express the effect on the whole plane CCBB, equal  $\pi d$ , the product of the weight  $\pi$  by the length  $d$  of the lever on which it acts in opposing the motion of the wheel.

§ III. If the wheel be plunged as deep as its axle, that is,  $f = o$ , the equation is changed into this  $\frac{\pi\beta\alpha\alpha}{6o} \left( \frac{1}{4} vv - \frac{2}{3} vz + \frac{1}{4} zz \right) = d\pi$ , where it appears

1°. That the quantities  $d$ ,  $\pi$ ,  $v$  and  $z$  remaining the same, we have  $\beta$  inversely proportional to the square of  $\alpha$ , whence it follows, that if the length  $\beta$  is to be diminished without altering the effect of the float-board, the height  $\alpha$  must be encreased proportionally to the square root of  $\beta$ ; for example, if  $\beta$  is to be made four times less, it will be sufficient to double the height  $\alpha$ . 2°. That likewise the velocity of the float-board remaining the same, the weight  $\pi$  will be in the compound ratio of the length  $\beta$ , and of the square of the height  $\alpha\alpha$ . 3°. Without meddling with the dimensions of the float-board, the more the quantity  $z$  is increased, the more must the weight  $\pi$  be diminished. If  $z$  be made  $= o$ , we have

$$\pi = \frac{\pi\beta\alpha\alpha}{6od} \cdot \frac{1}{4} vv, \text{ and if } z=v \text{ we have } \pi = \frac{\pi\beta\alpha\alpha}{6od} \cdot \frac{1}{4} vv,$$

that is six times greater than in the first case; which is very conformable to the nature of things, for when the wheel is in motion, the stream then not acting upon it but with the excess of its velocity above that of the wheel, it follows, that the greater such velocity is, the more will the effect of the stream be diminished.

It follows from our last remark, that the greatest weight with which the stream can constitute an equilibrium, will be  $= \frac{n\beta\alpha\alpha vv}{120d}$ , but then the wheel will not have any motion, nor consequently the weight  $\pi$ : If the float-board be increased, or the weight diminished, from that instant the wheel will begin to turn, and the swifter as the float-board is greater, or the weight less; but in most machines, it is required that the weight may be the greatest possible, as also the velocity with which it is raised. A question therefore here offers itself, whose solution is of much importance. What must be the velocity of the float-board whose dimensions are given, that the product of the weight by its velocity shall be the greatest possible?

§ IV. The velocity of the weight  $\pi$  is  $\frac{d}{\alpha} z$  feet in a second, which being multiplied by the value of  $\pi = \frac{n\beta\alpha\alpha}{60d} \left( \frac{1}{2} vv - \frac{2}{3} vz + \frac{1}{4} zz \right)$  will give the product  $\frac{n\beta\alpha}{60} \left( \frac{1}{2} v^2 z - \frac{2}{3} vzz + \frac{1}{4} z^3 \right)$  which must be a *maximum*; for which purpose make  $\frac{1}{2} vvdz - \frac{2}{3} vzzdz + \frac{1}{4} z^3 dz = 0$ ; whence we have  $z = \frac{8 - \sqrt{10}}{9} = 0,53752v$ : this value of  $z$  being substituted, make the equation  $\frac{1}{2} vv - \frac{2}{3} vz + \frac{1}{4} zz = \frac{11 + 2\sqrt{10}}{81} vv$ , so that we have the equation  $\beta\alpha\alpha = \frac{11 + 2\sqrt{10}}{4860} + \frac{d\pi}{\pi vv} = 280,529 + \frac{d\pi}{\pi vv}$ , which expresses the dimensions of the float-board where the effect will be the greatest possible. If the float-board be plunged no deeper than to CC, as we have at first supposed, the most advantageous value

of

of  $z$  may be determined in the same manner, which will be found

$$z = \frac{3a^4 - 8af^3 - a^4 \sqrt{10a^6 + 54a^4f^2 - 128a^3f^3 + 54a^2f^4 + 10f^6}}{9(a^4 - f^4)} v$$

If  $f = 0$ , this value of  $z = 0,537v$

$$f = 0,25 a = \frac{1}{4} a \quad = 0,498v$$

$$f = 0,3 a \quad = 0,486v$$

$$f = 0,5 a = \frac{1}{2} a \quad = 0,436v$$

$$f = 0,7 a \quad = 0,390v$$

$$f = 0,9 a \quad = 0,353v$$

$$f = a \quad = 0,333v$$

By the inspection of these different values it appears that this value of  $z$  diminishes as the plunged part is greater, and that this velocity can never exceed the quantity  $0,537v$ , nor be less than  $\frac{1}{3} v$  \*.

This value of  $z$  and of its square  $zz$  being substituted in the general formula (§ II.) we shall obtain from it the following equation :

$$\frac{60 d \alpha}{\alpha \beta v v}$$

$$= \frac{-27a^4f^2 + 32af^3 - 27a^2f^4 + 11f^6 + 2(a^3 - af)f \sqrt{10a^6 + 54a^4f^2 - 128a^3f^3 + 54a^2f^4 + 10f^6}}{81(a^4 - f^4)}$$

which for a given relation between  $f$  and  $a$  will shew the breadth  $\beta$  for producing the greatest effect.

\* If in the value of  $z$  we make  $f = a$ , we have  $z = \frac{0}{0}$

which obliges us to take, according to the common method, the differentials of the numerator and of the denominator, considering  $f$  as variable, and the relation of these differentials will give the value of  $z$ : but on account of the radical quantity, the calculus being somewhat tedious, and, again bringing out  $z = \frac{0}{0}$

and that after several similar operations, it is better to have recourse to the equation from which the value of  $z$  was deduced ;

$$\text{this equation is } \frac{1}{2} \alpha v z \frac{a^4 - f^4}{a^4 - f^4} - \frac{1}{2} \alpha \alpha v v \frac{a^2 - f^2}{a^4 + f^4}, \text{ which}$$

by the above operation will be  $\frac{1}{3} z \alpha = v \alpha - \frac{1}{3} v v$ , and  $z = \frac{1}{3} v$ .

As the extremity of the float-board must have a certain velocity depending on the relation of the height  $a$  to the plunged part, and as the velocity of the weight  $\pi = \frac{d}{a} z$ , it follows that if we would increase the velocity of  $\pi$ , we must diminish the height  $a$  and increase the breadth  $\beta$ , so that the product  $\beta a$  and the relation of  $f$  to  $a$  may be the same as before; for example, if the wheel be plunged as deep as the axle, to double the velocity of the weight, the height of the float-board must be reduced one half, and its breadth be quadrupled.

§ V. It may so happen that the channel on which the wheel is placed shall be so shallow and narrow, as not to allow the float-boards the necessary dimensions, for raising the weight with a convenient velocity. In this case we are obliged to raise the axle of the wheel above the surface of the water, so much that the lever on which the stream acts may be long enough to recompense the smallness of the float-boards. Herein it is necessary to solve the following problem:

*The breadth  $b$ , and the height  $a$ , of the float-board AB being given; to find the radius CA ( $r$ ) of the wheel which shall cause the weight  $\pi$  to ascend with the velocity  $\frac{d}{r+a} z$ .*

The exact solution of this problem might be deduced from the formula (§ II.) which would render the operation tedious, the equation being of the fourth degree; but it may be rendered far more simple by a supposition which is but little wide of the truth when AB is but small in comparison of CA, and this is to consider

consider all the points of the float-board AB as affected with the same velocity  $z$ .

Let CP fig. 3. =  $x$ , we shall have  $\frac{nb}{60} (v-x)^2 \int x dx$  for the effect of the portion AP, and  $\frac{nb}{60} (v-x)^2 \times \frac{2ar+aa}{2}$  for the effect of the whole float-board AB.

This quantity must be made equal to  $d\pi$ , and then, just as in the foregoing cases, such a value of  $z$  be sought, that the weight  $\pi$  and its velocity may be the greatest possible; that is, the differential of  $z(v-x)^2$  must be made = 0, which gives  $z = \frac{1}{3} v$ . There-

fore  $d\pi = \frac{nabvv}{270} (2r+a)$ , and  $r = \frac{270d\pi - naabvv}{2nabvv}$ , and the velocity of the weight  $\pi$  will be =  $\frac{2ndabvv}{810d\pi + 3nbaav} \times v$ .

§ VI. We have seen that the calculus was much simplified by supposing one of the velocities constant for all points of the float-board. For this velocity being  $c$ , the effect of the whole float-board will be simply  $\frac{n\beta}{60} (vc)^2 \frac{aa-ff}{2}$ . It will therefore not be

unuseful to inquire what this velocity  $c$  must be, that the effect of the float-board may be the same, as supposing, as we have hitherto done, a variable velocity, and proportional to the distances from the axle, we have only to make  $\frac{n\beta}{60} \frac{(v-c)^2}{(v-c)^2} \left( \frac{aa-ff}{2} \right) = \frac{n\beta}{60} \left( \frac{1}{3} vv(aa-ff) - \frac{2}{3} vz \frac{a^3-f^3}{a} + \frac{1}{3} zz \frac{a^4-f^4}{aa} \right)$

(§ IV.) but the equation whence we got the value of  $z$

As the extremity of the float-board must have a certain velocity depending on the relation of the height  $\alpha$  to the plunged part, and as the velocity of the weight  $\pi = \frac{d}{\alpha} z$ , it follows that if we would increase the velocity of  $\pi$ , we must diminish the height  $\alpha$  and increase the breadth  $\beta$ , so that the product  $\beta \alpha \alpha$  and the relation of  $f$  to  $\alpha$  may be the same as before; for example, if the wheel be plunged as deep as the axle, to double the velocity of the weight, the height of the float-board must be reduced one half, and its breadth be quadrupled.

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consider all the points of the float-board AB as affected with the same velocity  $z$ .

Let CP fig. 3. =  $x$ , we shall have  $\frac{n\beta}{60} (v-z)^2 \int x dx$  for the effect of the portion AP, and  $\frac{n\beta}{60} (v-z)^2 \times \frac{2ar+aa}{2}$  for the effect of the whole float-board AB.

This quantity must be made equal to  $d\pi$ , and then, just as in the foregoing cases, such a value of  $z$  be sought, that the weight  $\pi$  and its velocity may be the greatest possible; that is, the differential of  $z (v-z)^2$  must be made = 0, which gives  $z = \frac{1}{3} v$ . Therefore  $d\pi = \frac{nabvv}{270} (2r+a)$ , and  $r = \frac{270 d\pi - naabvv}{2nabvv}$ , and the velocity of the weight  $\pi$  will be =  $\frac{2ndabvv}{810d\pi + 3nbaav} \times v$ .

§ VI. We have seen that the calculus was much simplified by supposing one of the velocities constant for all points of the float-board. For this velocity being  $c$ , the effect of the whole float-board will be simply  $\frac{n\beta}{60} (vc)^2 \frac{aa-ff}{2}$ . It will therefore not be unuseful to inquire what this velocity  $c$  must be, that the effect of the float-board may be the same, as supposing, as we have hitherto done, a variable velocity, and proportional to the distances from the axle, we have only to make  $\frac{n\beta}{60} \frac{(v-c)^2}{(v-c)^2} \left( \frac{aa-ff}{2} \right) = \frac{n\beta}{60} \left( \frac{1}{3} vv (aa-ff) - \frac{1}{3} vz \frac{a^3-f^3}{3} + \frac{1}{3} zz \frac{a^4-f^4}{4} \right)$  (§ IV.) but the equation whence we got the value of  $z$

(§ IV.)  $\frac{1}{2}vv - \frac{4vz}{3a} \cdot \frac{a^3 - f^3}{a^2 - f^2} + \frac{3}{4}zz \cdot \frac{aa + ff}{aa} = 0$ , we shall have  $c = v - \sqrt{\frac{1}{2}vv - \frac{3}{4}zz \cdot \frac{aa + ff}{aa}}$ .

If  $f = 0$ , we have  $zz = 0,288vv$  and  $c = 0,345v$

$f = \frac{1}{2}a$   $zz = 0,190vv$   $c = 0,336v$

$f = a$   $zz = \frac{1}{2}vv$   $c = \frac{1}{3}v$ .

so that whatever be the relation of  $f$  to  $a$ , the velocity  $c$  is ever nearly  $= \frac{1}{3}v$ , and the more exactly so, as  $f$  is greater. Wherefore we may always assume  $\frac{1}{2}vv - n\beta vv.(aa - ff) = d\pi$  for the effect of the stream upon a float-board whose plunged part is  $a - f$ ; this effect will be increased in the ratio of 4 to 9, when the wheel has no motion, for making  $c = 0$ , we find it  $= \frac{1}{2}vv - n\beta vv.(aa - ff)$ .

§ VII. Hitherto we have all along supposed, that the float-board did through its whole plunged part receive the perpendicular impulse of the stream; but it is easily understood, that the wheel coming to turn, presents to the stream the plane of the float-board under an angle which is continually varying, which diminishes its effect every instant, as it removes from the vertical: This inconvenience may be remedied by multiplying the number of the float-boards, so that when the first is removed from the vertical as far as a certain point, the next may occupy that advantageous place, to be in its turn replaced some time after by a third, and so on. Now our third inquiry is, to assign the angle contained between two float-boards, or, which comes to the same, the number of float-boards the wheel should consist of, that its effect may be the greatest possible, being of no less importance than the preceding ones. To begin then with the most simple

simple case; we will suppose the wheel immoveable, or that  $c = 0$ , and proceed to investigate, whether, supposing the number of float-boards to be greater, the sum of the effects will come out greater or less than what results from one single float-board placed vertically.

In order to a general solution of this question, we will suppose two float-boards CD and CE fig. 4. making any angles with the vertical, and let us compare the effect of the single float-board GD with the effect resulting from the float-boards FE and GD taken together, which will be reduced to FE and OD, because the part OG becomes useless, as the stream is intercepted by FE. Let  $CB = CD = CE = a$ ,  $CA = f$ ,  $\cos \angle BCD = m$ ,  $\cos \angle BCE = \mu$ , which gives  $CG = \frac{f}{m}$   $CF = \frac{f}{\mu}$  and  $CO = \frac{\mu}{m} a$ . Then we shall find, by § VI, the effect of GD  $= \frac{n\beta vv}{120} (mmaa - ff)$  that of OD  $= \frac{n\beta vv}{120} (mmaa - \mu\mu aa)$ . and that of FE  $= \frac{n\beta vv}{120} (\mu\mu aa - ff)$ ; whence it appears, that the sum of the two last is exactly equal to the first, which will ever hold good whatever be the value of  $f$ .

Whence arises the following theorem:

*Whether the wheel be plunged quite up to the axle, or only in part so, provided it be immoveable, and that one of its float-boards be placed vertically, its effect will be constantly the same, whatever be the number of float-boards opposed to the stream, even though it were infinite.*

The

The latter part of this theorem, though flowing from the general demonstration, may be also demonstrated, immediately, in the following manner; Let BP fig. 5. be

$$= x, \text{ we have } MO = \frac{a dx}{a-x}, \text{ and } CO = \frac{aa - dx - ax}{a-x};$$

$$\overline{CO}^2 = \frac{a^4 - 2a^3x + a^2x^2 - 2a^3dx + 2aaxdx}{aa - 2ax + xx} \text{ neglect-}$$

ing the  $dx^2$ ) and  $aa - \overline{CO}^2 = \frac{2aaxdx}{a-x}$ ; Therefore

$$\text{the effect of the stream upon OM, which is } = \frac{n\beta vv}{120} \cdot (aa - \overline{CO}^2) \cdot \frac{\overline{CP}^3}{\overline{CP}}$$

will become  $= \frac{n\beta vv}{120} (2ax - 2x dx)$  whose integral is  $= \frac{n\beta vv}{120} (2ax - xx)$  where-

in putting  $x = a - f$ , we have  $\frac{n\beta vv}{120} (aa - ff)$  for the total effect of the stream upon the wheel, which is the same as that of a single float-board AB in a vertical position.

§ VIII. This theorem will also hold true for the case of § V. wherein we have supposed the height of the float-boards very small, in comparison of the radius of the wheel; we have seen that the effect of a single float-board placed vertically was  $= nabvv (2r + a)$ ; the demonstration of the preceding § will be applicable here after the same manner, and will shew that whatever be the number of float-boards, the effect will be ever the same.

It does not however follow that the number of float-boards should be indifferent; for the wheel coming to turn the float-board, its lower part, which received the perpendicular impulse, will no longer

receive it otherwise than obliquely, and the effect will diminish till the angle formed by two neighbouring float-boards be bisected exactly by the vertical, which will render the first entirely useless; after which the effect will increase anew, and will become again greatest, when the second float-board is got to the vertical; so that in order to fix upon the most advantageous number of float-boards, regard must be had to the sum of the different effects for all the situations of the float-boards during one whole turn of the wheel.

Whence it follows that in this case, wherein they are supposed very small, the greater their number is, the greater will be the sum total of the effects, since, if that number were infinite, there would be a float-board in a vertical position every instant.

§ IX. This will no longer hold good, if the height of the float-boards be more considerable, and it be found necessary to take the different velocity of their different points into consideration; by comparing (fig. 4.) the pressure on FE with that on the portion GO, they will be found no longer equal, as in the foregoing case; it is true that the same quantity of fluid acts on these two planes, and the disadvantage which FE has by receiving the impulse more obliquely, is exactly compensated, as before, by the length of the lever, but the difference arises from the different velocity of the corresponding points of FE and GO; those velocities are in the ratio of CF to CG, or as  $\cos. ACG$  to  $\cos. ACF$ , which shews that the effect of FE is always less than that of GO, and consequently the effect must be diminished, by adding a greater number of float-boards, the

the said effect will be greatest when there is only one float-board placed vertically, and least when their number is infinite: let us enquire what it will be in this latter case. We will suppose the same fig. 5, and the same denominations as in § VII. We had  $CO = \frac{aa - ax - adx}{a - x}$ , we shall have (neglect-

$$\text{ing } dx^2, dx^3, \text{ and } dx^4) \quad aa - \overline{CO} = \frac{2axdx}{a-x},$$

$$\frac{a^3 - \overline{CO}^3}{a}, \text{ and } \frac{a^4 - \overline{CO}^4}{aa} = \frac{4axdx}{a-x}. \text{ Now the pref-}$$

$$\text{sure on OM is, by § II, } = \frac{n\beta}{120} \frac{a-x}{aa} \left( vv(aa - \overline{CO}^2) - \frac{4}{3} vz \cdot \frac{a^3 - \overline{CO}^3}{a} + \frac{1}{2} zz \cdot \frac{a^4 - \overline{CO}^4}{aa} \right)$$

which (by putting for CO its value) will become  
 $= \frac{n\beta}{120} (2adx - 2xdx) (v - z^2)$  whose integral

$$\frac{n\beta}{120} (2ax - xx) (v - z^2) = \frac{n\beta (v - z^2)}{120} (aa - ff)$$

(making  $x = a - f$ ) will express the effect resulting from an infinite number of float-boards: this least effect will be to the greatest, that is when there is but one float-board, as  $(v - z^2) : vv - \frac{4}{3} \frac{vz}{a} \cdot \frac{1-f^3}{aa-ff} + \frac{1}{2} \frac{zz}{aa} (aa + ff)$  or as  $(v - z^2)$

$$: \frac{1}{2} vv - \frac{1}{2} zz \cdot \left( \frac{aa + ff}{aa} \right) \text{ §. VI.}$$

This ratio will be that of 1 : 2

$$1 : 1,485$$

$$1 : 1$$

if  $f = 0$

$$f = \frac{1}{2}a$$

$$f = a$$

§ X. If

§ X. If we take nothing but the most advantageous position into consideration, and preserve the greatest effect entire, it follows that the angle BCD (fig. 6.) between two float-boards must be such that E should enter the water at the instant when AB quits the vertical, so that the cosine of that angle be  $= \frac{f}{a}$ ; in consequence of which the following table may be constructed, shewing what the number of float-boards should be for a given ratio between  $f$  and  $a$ .

For 4 float-boards, we have  $f = 0$ ,

5	0,3090a
6	0,5000a
7	0,6236a
8	0,7071a
9	0,7660a
10	0,8090a
12	0,8669a
14	0,9009a
16	0,9239a
18	0,9397a
20	0,9510a
Ec.	Ec.

§ XI. Certain authors treating of hydraulics, have in this part thereof given the same table, as containing the true number of float-boards the wheel should consist of: but we have seen upon what principle it was formed, and that it was only to preserve entirely the effect of the vertical float-board; from whence it follows not that the number of float-boards which it assigns should be the most advantageous.

geous. To which purpose the effect produced from every position of the wheel, and for the different number of the float-boards, should be computed; the number which gives the arithmetical mean between all these effects, the greatest of all, will be that to be chosen, and preferred before what the above table indicates.

It may be sufficiently satisfactory to compute only the effect from 1 to 10 degrees. Thus, for example, for the wheel entirely plunged, we are to find the effect (fig. 7.)

1° on OA, 2° on OI and gb, 3° on OH, and fc, 4° on OG and bd, 5° on OF, and pe, 6° on OE, 7° on OD, 8° on OC, and 9° on OB.

After which the wheel returns into the same position it had at first; and we are to divide the sum of all these effects by 9, to get the arithmetical mean.

We will next suppose the number of six float-boards for the same case of  $f=0$ , and compute the following effects.

1° on OG + aA, 3° on OE + nc, 5° on OI + yC  
2° OF + mb, 4° OD, 6° OH + zB.

The sum of all these effects divided by 6 will give the effect of the wheel of 6 float-boards.

The same thing, supposing the angle 40 degrees, or 9 float-boards, and as after a revolution of these 40 degrees, the wheel returns into a similar position, the same must be divided by 4.

Then for an angle of 30 degrees we are to divide by 3, and so on.



I have made this computation to great exactness, for the case of  $f = 0$ ,  $f = \frac{1}{2} \alpha$ , and  $f = \alpha$ ,  $866 \alpha = \alpha \cos. 30^\circ$ ; the result,

1° If  $f = 0$ .  
for 4 float-boards, the arith. mean  $\equiv 0.335$   
( $\frac{1}{3} n \beta \alpha \alpha v$ ).

It may be observed in this first case, that there is some advantage in taking 6 float-boards instead of 4 shewn by the table; the effect will be increased in the ratio of 100 to 118, and yet will be more than about  $\frac{2}{100}$  of the greatest effect above calculated for a single vertical float-board; so that the found dimensions must be a small matter altered, and the quantity  $\beta \alpha \alpha$  increased by  $\frac{1}{10}$ .

for 6	$\equiv 0.396$ (C.
9	$\equiv 0.336$ (C.
12	$\equiv 0.323$ (C.
18	$\equiv 0.295$ (C.
an infinite number	$\equiv 0.214$ (C.

2° If  $f = \frac{1}{2} \alpha$ .

for 6 flo.	$0.277$ (C.
7	$0.281$ (C.
9	$0.285$ (C.
12	$0.284$ (C.
18	$0.276$ (C.
an infinite number	$0.238$ (C.

In this second case 9 float-boards are to be taken instead of 6 shewn by the table, though the difference will be but very small; and we shall have an effect which will be  $\frac{3}{100}$  of that of a vertical float-board, and in that ratio that the quantity  $\beta \alpha \alpha$  found by the above formulæ, must be increased.

3° If  $f = 0,866 a$

for 12 fl.

18

36

an infinite number

$= 0,099$  (Gr.

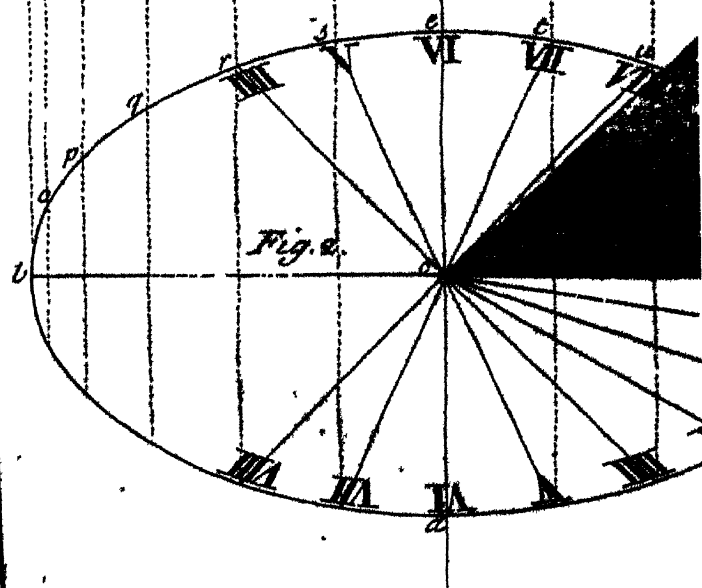
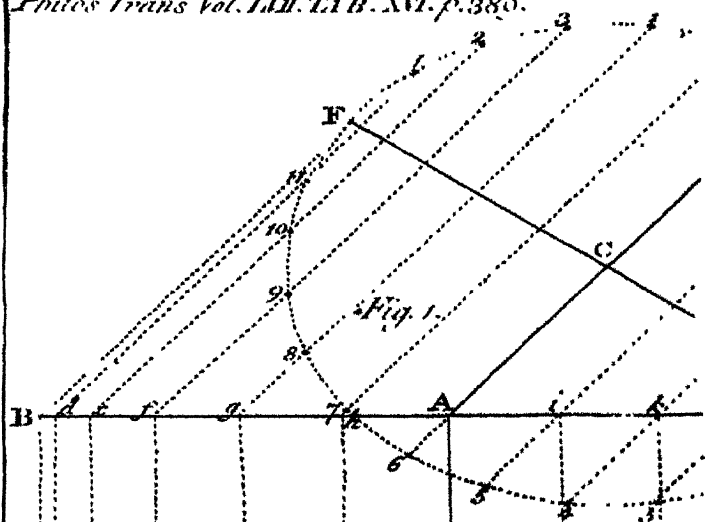
$= 0,099$  (Gr.

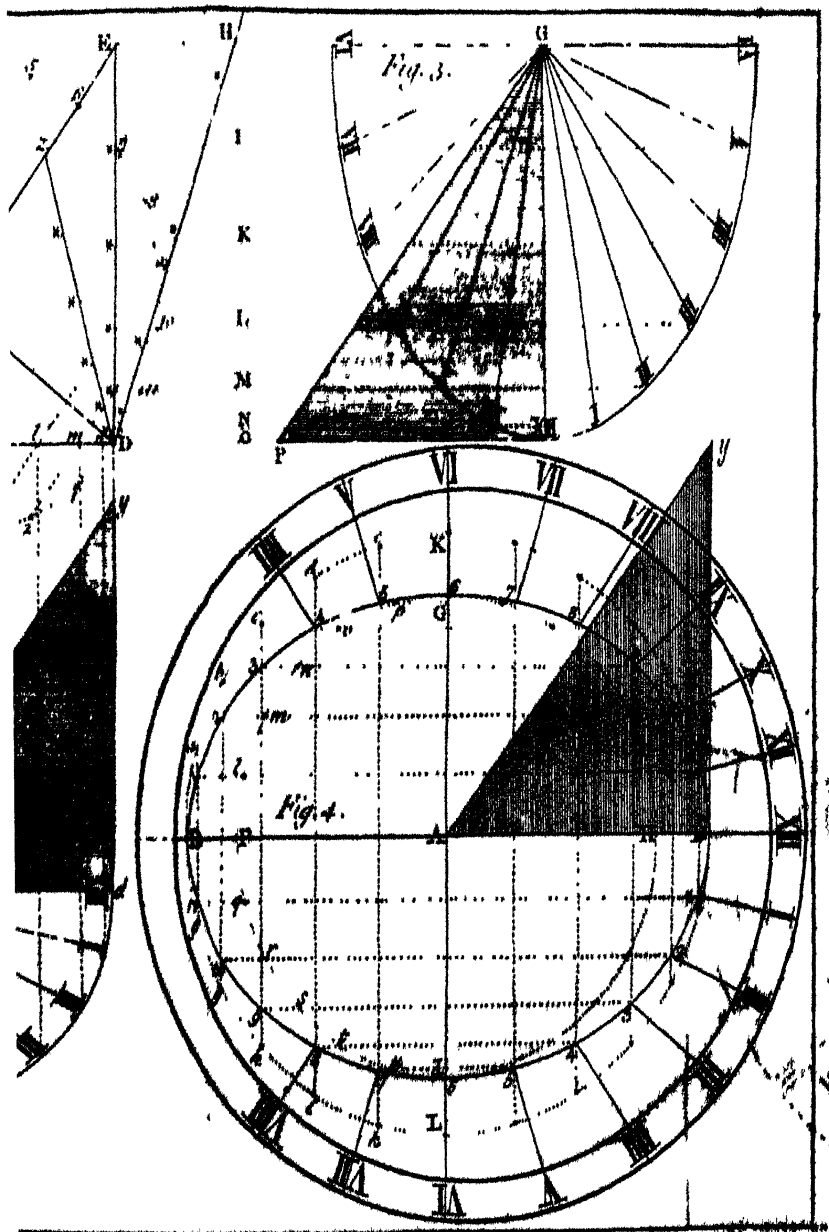
$= 0,104$  (Gr.

$= 0,103$  (Gr.

In this third case the difference is still very small, and the effect resulting from 36 float-boards will be  $\frac{0.3}{10.2}$  of the effect of a single vertical float-board.









XXXVI. *A new Method of constructing Sundials, for any given Latitude, without the Assistance of Dialing Scales or Logarithmic Calculations.* By James Ferguson, F. R. S.

Read July 2, 1767. **D**RAW the straight horizontal line BAD TAB. XVI. (fig. 1.) of any convenient length, and on the end D thereof raise the perpendicular DE.

Bisect BAD at A, and draw the right line ACE, making the angle EAD equal to the latitude of the place for which the dial is to serve, as suppose  $51^{\circ}$ ; for the latitude of London. Draw also the right line FCD, making an angle at D with the horizontal line BA equal to the altitude of the place, or height of the sun at noon. FCD will be perpendicular to ACE, BAD will be a horizontal plane (seen edgewise), DE a vertical plane, FCD the plane of the equinoctial, and ACE the axis or stile of the dial; the whole triangle ADE representing the whole front plate of stile.

Around the intersection C, as a center, with the radius CD, describe the circle FGDGF, and divide its circumference into 24 equal parts, beginning at D and at F. Then connect all the points of division, which are marked on F, by the straight lines 1 11, 2 10, 3 9, &c. continuing as many of

these lines as are needful to the horizontal line  $BAD$ , and to the vertical line  $DE$ .

Continue  $ED$  down to  $d$  (fig. 2.) and draw  $bd$  parallel and equal to  $BD$ . Draw also the right line  $Aeca$ , from fig. 1. to fig. 2, and that the line will be perpendicular to the line  $bd$  in fig. 2. cutting it in the middle point  $c$ .

From fig. 1. take  $CF$  or  $CD$  in your compasses; and in fig. 2. set that distance from  $c$  to  $e$  upwards, and from  $c$  to  $a$  downwards, on the right line  $Aeca$ . So  $eca$  in fig. 2. shall be equal to  $FCD$  in fig. 1. and  $bcd$  in fig. 2. shall be equal to  $BAD$  in fig. 1.

On these two lines  $bcd$  and  $eca$  make the ellipsis  $bopqr$ , &c. according to the common rule for describing an ellipsis upon the transverse and conjugate diameters  $bcd$  and  $eca$ . Then, from those points in the horizontal line  $BAD$  (fig. 1.) where the right lines 1 11, 2 10, 3 9, &c. meet it, as at  $d, e, f, g, h, A, i, k, l, m, n$ , draw the right lines  $de, ep, fq, gr$ , &c. quite through the ellipsis, and all parallel to the right line  $Aeca$ . Then, from the middle point  $c$  of the ellipsis, draw right lines to those points of its circumference where the foresaid parallel lines cut it; and they shall be the true hour-lines for a horizontal dial, to which set the hours, as in fig. 2. Lastly, in fig. 2. draw  $cy$  parallel to  $ACE$  in fig. 1. and  $cy$  shall be the axis or edge of the stile  $cdy$  that casts the shadow on the face of the dial.

The horary spaces, or angular distances of the hours on the dial, being thus found, there is no occasion for engraving the hours thereon, or the lengths of the hour-lines, within the ellipsis; for they may be



produced beyond it to any distance, and the hour-letters placed in a circle, as in fig. 4.

A geometrical method for describing the whole or half an ellipsis will be shewn further on.

From fig. 1. continue out the horizontal line BAD to any length, as to XII in fig. 3. Then, from the points \*\*\* in the perpendicular DE (fig. 1.) where the parallel lines 5 7, 4 8, 3 9, 2 10, and 1 11 meet it, draw the right lines H, I, K, L, M, N, all parallel to the horizontal line BADP XII. producing them at pleasure: and, in fig. 3. draw GXII parallel to DE in fig. 1. This done, take CF or CD (fig. 1.) in your compasses, and set off its length both ways from G (fig. 3.) to VI and VI, on the right line EH VI G VI. So VI G VI in fig. 3. shall be equal to FCD in fig. 1. and XII G in fig. 3. shall be equal to DE in fig. 1.

On VI G VI as a conjugate diameter, and G XII as a semi-transverse diameter, describe the semi-ellipse VI, VII, VIII, IX, X, XI, XII. Then, from the points VI, VII, VIII, IX, X, XI, XII, draw the right lines G VI, G VII, G VIII, G IX, G X, G XI, G XII, as in the figure; and they will be the true hour-lines for an erect direct fourth dial: and they may be produced beyond the ellipse, and limited either by circular or square lines, between which the hours may be placed.

Lastly, draw PG in fig. 3. parallel to DE in fig. 1. and PG will be the axis or edge of the dial XII G XII being a shadow line.

And this dial may be constructed for any given latitude, and may be made for the latitude.

If you want a south dial to incline by any number of degrees, as suppose 16, draw the line  $Dz$ , making an angle of 16 degrees with the perpendicular  $DE$ , in fig. 1. Then  $Dz$  shall be the semi-transverse axis of the ellipse, and  $C6$  the semi-conjugate: and right lines drawn parallel to  $DP$  XII quite through the semi-ellipsis, from the points \*\*\* in  $Dz$ , where it is cut by the parallel lines 5 7, 4 8, 3 9, &c. shall cut the semi-ellipsis in those points through which the hour-lines must be drawn, as from  $G$  in the upright south dial, fig. 3.

If you want to make a reclining south dial, draw the line  $DH$  (fig. 1.) making an angle with the perpendicular  $DE$  equal to the intended angle of reclination, and produce  $DH$  and  $CE$  till they meet. From  $D$  to that meeting, will be the length of the semi-transverse axis of the ellipse, and from  $C$  to 6 the length of the semi-conjugate: which being found, proceed in all respects as above for the south upright dial.

To draw the ellipse, and find the hour-points in it, observe the following method.

For a horizontal dial, as fig. 4. Make the radius  $AK$  of the circle  $BKDL$  equal to  $AD$  in fig. 1. and cross the circle at right angles by the two diameters  $BAD$  and  $KAL$ , and divide the circle into 24 equal parts, beginning at  $B$ . Connect these points of division, which are equidistant from  $B$ , by the right lines  $af$ ,  $bg$ ,  $ch$ , &c. all parallel to  $KAL$ , as in the figure.

Make the radius  $AG$  of the circle  $FGHI$  in fig. 4. equal to  $CF$  in fig. 1. and divide  $FGHI$  into 24 equal parts, beginning at  $I$ . Then through these points

points of division, which are equidistant from I, draw the right lines 7 5, 8 4, 9 3, 10 2, &c. till they meet the former right lines, *ek*, *di*, *cb*, &c. in the points 7 5, 8 4, 9 3, 10 2, and 11 1, on both sides of the diameter BAD; all which points are in the elliptical curve, and it is to be drawn through them, by hand, as in the figure.

And right lines drawn from the center A through these points in the ellipsis, will be the true hour lines for a horizontal dial.

To draw the ellipsis for a vertical south dial, make DE (in fig. 1.) the radius of the largest circle, and CF the radius of the smallest: the diameter of the former gives the transverse diameter of the ellipsis, and that of the latter gives the conjugate: which being found, construct the ellipsis the same way for the vertical dial as above shewn for the horizontal; then draw the hour-lines in the same manner, from the center of the dial, through those points of the ellipsis where the intersections of the cross-lines meet it, as in the horizontal; and the thing will be done.

XXXVII. *On the Formation of Islands.* By  
Alexander Dalrymple, *Esquire.* Com-  
municated by C. Morton, M. D. S. R. S.

Received May 4, 1767.

Read July 2, 1767. **T**HERE is not a part of natural history more curious, or perhaps to a navigator more useful, than an enquiry into the formation of islands. The origin of islands, in general, is not the point to be discussed; but of low, flat, islands in the wide ocean; such as are most of those hitherto discovered in the vast South-sea.

These islands are generally long, and narrow; they are formed by a narrow bar of land, inclosing the sea within it; generally, perhaps always, with some channel of ingress at least to the tide; commonly, with an opening capable of receiving a canoe; and frequently sufficient to admit even larger vessels.

The origin of these islands will explain their nature. What led me first to this deduction was an observation of Abdul Roobin, a Sooboo pilot; that all the islands, lying off the N.E. coast of Borneo, had shoals to the eastward of them.

These islands being covered to the westward by Borneo; the winds from that quarter do not attack them

them with violence. But the N.E. winds, tumbling in the billows from a wide ocean, heap up the coral with which those seas are filled. This, obvious after storms, is perhaps, at all other times, imperceptibly effected.

The coral banks, raised in the same manner, become dry. These banks are found of all depths, at all distances from shore, entirely unconnected with the land, and detached from each other : although it often happens they are divided by a narrow gut, without bottom.

Coral banks also grow, by a quick progression, towards the surface ; but the winds, heaping up the coral from deeper water, chiefly accelerate the formation of these into shoals and islands. They become gradually shallower ; and, when once the sea meets with resistance, the coral is quickly thrown up by the force of the waves breaking against the bank ; and hence it is that, in the open sea, there is scarce an instance of a coral bank having so little water, that a large ship cannot pass over, but it is also so shallow that a boat would ground on it.

I have seen these coral banks in all the stages ; some in deep water, others with few rocks appearing above the surface, some just formed into islands, without the least appearance of vegetation, and others, from such as have a few weeds on the highest part, to those which are covered with large timber, with a bottomless sea, at a pistol shot distance.

The loose coral, rolled inward by the billows in large pieces, will ground, and the reflux being unable to carry them away, they become a bar to coagulate the sand, always found intermixed with

coral; which sand, being easiest raised, will be lodged at top. When the sand bank is raised by violent storms, beyond the reach of common waves, it becomes a resting place to vagrant birds, whom the search of prey draws thither. The dung, feathers, &c. increase the soil, and prepare it for the reception of accidental roots, branches, and seed, cast up by the waves, or brought thither by birds. Thus islands are formed: the leaves and rotten branches, intermixing with the sand, form in time a light black mould, of which in general these islands consist, more sandy, as less woody; and when full of large trees, with a greater proportion of mould.

Cocoa nuts, continuing long in the sea without losing their vegetative powers, are commonly to be found in such islands; particularly as they are adapted to all soils, whether sandy, rich, or rocky.

The violence of the waves, within the Tropicks, must generally be directed to two points, according to the monsoons.

Hence the islands formed from coral banks must be long and narrow, and lie nearly in a meridional direction. For even supposing the banks to be round, as they seldom are when large, the sea, meeting most resistance in the middle, must heave up the matter in greater quantities there than towards the extremities: and, by the same rule, the ends will generally be open, or at least lowest. They will also, commonly, have soundings there, as the remains of the bank, not accumulated, will be under water.

Where the coral banks are not exposed to the common monsoon, they will alter their direction; and

and be either round, extend in the parallel, or be of irregular forms, according to accidental circumstances.

The interior parts of these islands, being sea, sometimes form harbours capable of receiving vessels of some burthen, and, I believe always, abound greatly with fish; and such as I have seen, with turtle-grass and other sea-plants, particularly one species, called by the Sooloos Gammye, which grows in little globules, and is somewhat pungent, as well as acid, to the taste.

It need not be repeated, that the ends of those islands, only, are the places to expect soundings: and they commonly have a shallow spit running out from each point.

Abdul Roobin's observation points out another circumstance, which may be useful to navigators; by consideration of the winds to which any islands are most exposed, to form a probable conjecture which side has deepest water; and from a view which side has the shoals, an idea may be formed which winds rage with most violence.

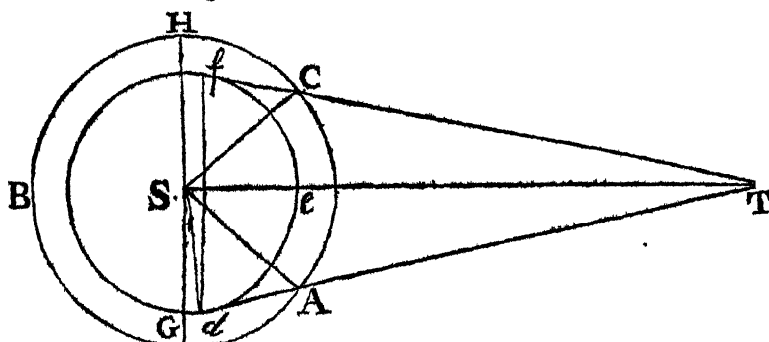
XXXVIII. *An Attempt to determine the Height of the Sun's Atmosphere from the Height of the Solar Spots above the Sun's Surface: In a Letter to Mr. J. Ellicot, F. R. S. from the Rev. Mr. Horfeley, F. R. S.*

Read July 9, 1767. **I** Return you many thanks for your obliging communication of the observations of the late transit of Venus by Mayer and Rohlius. The phenomena which they relate of the atmosphere of that planet are highly curious. They were perhaps the more interesting to me, as they confirmed some conjectures of my own, concerning the great height of the atmosphere of the sun, and of those of the two nearer planets. I once attempted to make a rough comparison between the height of the sun's atmosphere and that of our own, by comparing the height of the solar spots above the surface of the sun with that of our clouds above the surface of the earth, which I did in the following manner.

The inclination of the Sun's equator to the plane of the earth's orbit is so small, that in this enquiry I think it may safely be neglected; and I consider the two planes as one. Let T be the center of the earth, S that of the Sun. Join TS, and let *def* be a great circle of the sun's sphere, formed by the intersection of the plane of the earth's orbit with the sun's surface.



face. Let ABC be the circle described by the revolution of a spot. From T draw Tf and Td touch-



ing the circle *def* in *f* and *d*, and cutting ABC, in C and A. Join *df*; through S draw HSG, parallel to *df*. Join SC, SA, Sd. The spots are hid behind the sun three days longer than they are visible. That is, they are hid 15 days, and are seen only 12. The earth's motion in 15 day is  $14^{\circ} 17'$ . The spots traverse the like area in 1 d. 0 h. 50' nearly. Hence, if the earth stood still, the spots would be hid only 13 d. 23 h. 10', and their whole sideral period being 25 d. 5 h. they would be visible 11 d. 5 h. 50', and the time of their occultation would exceed the time of their appearance by 2 d. 17 h. 20'. Hence the arc AC is less than the arc ABC, by the motion of 2 d. 17 h. 20', that is, by  $38^{\circ} 52' 56''$ . And the semi-circle being a mean arithmetic between AC and ABC, AC will be less than the semi-circle by half as much; that is, by  $19^{\circ} 26' 28''$ . Hence each of the angles GSA, HSC is  $9^{\circ} 43' 14''$ . The angle  $bsd = dTs = 10^{\circ} 1' 27''$ . Therefore  $dSA$  is  $9^{\circ} 27' 12''$ . Hence  $SA = 1,013,767$  such parts as Sd is 1.

The

The distance therefore of these spots from the center of the sun is 1,013767 semi-diameters of the sun, and their distance from his surface is in decimal parts of his semi-diameter ,013767. Hence it is evident that the height of the solar spots above the surface of the sun, is above 54 such parts, as bear each to the sun's semi-diameter, the proportion of one Paris mile to the semi-diameter of the earth, which is that of 1 to 3923 nearly. The height of our atmosphere is generally reckoned about 50 miles. That of the lightest clouds fall short of one mile. The whole height of our atmosphere therefore is, at least, 50 times that of our highest clouds. If the whole height of the sun's atmosphere bear as large a proportion to the height of these solar spots or clouds (and I think the proportion is likely to be much larger), the height of the sun's atmosphere is not less in proportion to his semi-diameter, than 54 times that of the earth's, and exceeds two thirds of his semi-diameter, being in decimal parts thereof ,68835.

The probability seems to be that the height of the sun's atmosphere is almost double of this: for I question whether the mean height of our clouds exceeds a Paris mile. The solar spots, therefore, are 108 times as high in proportion; and then, supposing as before, that the whole height of the sun's atmosphere bears the same proportion to the height of his spots, as the whole height of our atmosphere to the mean height of our clouds, the sun's atmosphere will be 108 times as high in proportion to his semi-diameter as ours is, and will rise

to the distance of more than  $\frac{4}{5}$  of his semi-diameter from his surface.

Let philosophers consider, whether these indications of the vast height of the sun's atmosphere give any degree of probability to a conjecture of Sir Isaac Newton's, that the dissipation of the sun's substance, which might be expected to ensue from his intense heat, may in great part be prevented by the prodigious pressure of the incumbent atmosphere.

The height of the atmosphere of Venus is considerably greater according to the observations of Mayer and Rohlius than they imagined. Rohlius follows Cassini in the estimation of the sun's apogee semi-diameter, which Cassini over rated by  $3'' 45'''$ . This quantity, therefore, is to be added to the height of Venus's atmosphere (15,5) as stated by Rohlius; which makes the true height  $19''.25$ , that is above  $\frac{1}{3}$  of the diameter of the planet. I cannot but reflect with some degree of national triumph on the great part that our own country may justly claim in many of the most curious discoveries in all parts of the world. Mr. Meyer generously confesses how much he stood indebted to English artists. You told me that it is your intention to present that curious tract to the Royal Society.. You may likewise communicate this if you think it contains any thing worthy of their notice.

I am, Sir, with great esteem,

Your most obedient

and most humble servant,

Broad-Street,  
June 11, 1767.  
VOL. LVII.

F f f

Sam. Horsley.  
XXXIX.

XXXIX. *Observations of the Sun's Eclipse,*  
*16th of August, 1765, taken at Caën in*  
*Normandy. By Nathanael Pigott, Esquire,*  
*of Whitton, in Middlesex. Communicated*  
*by J. Bevis, M. D. F. R. S.*

Read July 9, 1767.

Tr. Time.

	h	'	"				h	'	"	
At 3	57	28	the seg. of the sun's illum. diam. meas.	29	38	14				
4	8	52	the distance of the horns	ditto	14	47	37			
4	18	39	the seg. of the illuminated diam. ditto	27	4	35				
4	24	28	the distance of the horns	ditto	16	20	24			
4	35	47	the seg. of the illuminated diam. ditto	27	14	14				
4	43	4	the distance of the horns	ditto	14	26	6			
4	52	38	the seg. of the illuminated diam. ditto	29	52	1				
4	56	54	the distance of the horns	ditto	7	46	4			

		h	'	"		h	'	"		
Sun's incl. diam. meas.	at	3	19	38		31	45	11	} the mean ' " "	
— Ditto —	at	3	22	10		31	42	58		
Sun's horiz. diam. aft.	}	at	5	31	33		31	41	29	} 31 43 20 of the Sun's diam. meas.
the Eclipse										
Sun's inclined diam.	}	at	3	53	37		31	43	42	
meas. August 15 <sup>th</sup>										

Eclipse

Eclipse beg. tr. time at	3 48 16	} hence the mid. was at and greatest phase obs. at whence the ecl. incr. for	4 24 36
— end.	at 5 0 56 $\frac{1}{2}$		4 18 39
— mid.	at 4 24 36		
— dur.	at 1 12 40 $\frac{1}{2}$		5 57 of

time, in which the Sun's diam. illum. decreased  $36'' 14'''$ ; there-

fore from the Sun's diam. illuminated at 4 18 39 = 27 4 35  
take the decrease in + 5 57 = 0 36 14

the diam. of the Sun at the middle 4 24 36 = 26 28 21

which taken from the mean diam. meas. } 5 14 59  
31' 43" 20''' gives

the quantity of the eclipse, or segment of the diameter eclipsed, which is 1 digit and 59', 15 of a digit, or  $1 \frac{59}{60} = 2$  digits nearly. This eclipse was observed with an achromatic refractor of 6 feet, and a micrometer made by Dollond. The weather very fine.

The times, as computed from the Tables at the end of  
M. De la Lande's Astronomy.

	h	'	"		h	'	"	difference.
Beginning at	3	48	24,6	observ. at	3	48	16	0 8,6
Middle	4	25	11,0	—	4	24	36	0 35,0
End	5	1	57,2	—	5	0	56,5	1 0,7
Duration	1	13	32,6	—	1	12	40,5	0 52,1

Also the latitude of the Moon was, by observation, 16" greater than the tables gave it.

XL. *Extract of a Letter from John Ellis, Esquire, F. R. S. to Dr. Linnæus, of Upsal, F. R. S. on the Animal Nature of the Genus of Zoophytes, called Corallina.*

Read July 9, 1767. **I** HAVE now finished a collection of that genus of Zoophytes, which you call Corallina; and, with the assistance of our learned friend Dr. Solander, have made a description of each species: to do this with more exactness, I have taken care to dissect them minutely, and to pass them in review under his eye in the microscope, in order to establish a true general character of this genus.

I have attended more particularly to examine the nature of these bodies, in order to confute the opinions of some late writers on Zoophytes, who, for want of good microscopes, and a proper care in chemically analysing them, have asserted that they were mere vegetables.

The first of these is Dr. Job Baster, of Zeeland, who, in the Philosophical Transactions, Vol. LII. p. 111, asserts that the Corallines of Linnæus, which he says he has accurately examined, are most evidently true plants of the genus of Conferva; because there are no polypes coming out of their tops, and that they have feed inclosed in their cells like other marine

rine plants \*. But, as another part of this letter is intended for an inquiry into this new discovery of Dr. Baster's, that Corallines are Confervas; a thing never known even to the great Mr. Ray, Dr. Dillenius, or any other botanist, I shall now proceed to his ingenious friend Dr. Pallas of Berlin, who has lately resided in Holland, and who has taken great pains in collecting every thing that has been wrote on the subject of Zoophytes, from whence he has compiled a book called *Elenchus Zoophytorum*, where he has ranged the several genera and species of this class of beings in a systematical order.

When he comes to the genus of *Corallina*, he says (vide Pallas *Elenchus*, p. 418.) †, "They are to be left to the botanists, as they belong to the vegetable kingdom; but makes this apology for inserting them, least his book should be thought imperfect, as Linnæus and Ellis have ranked them as Zoophytes in their works."

\* *Corallinas*, non *Zoophyta*, quamvis Linnæus iisdem adnumeret, sed veras e confervarum genere plantas esse, luculentissime perspexi. Nunquam in earum apicibus polypi inveniuntur: semen contra cellulis inclusum eodem quo aliæ plantæ marinæ modo produnt. Phil. Trans. Vol. LII. p. 3.

† *Corallinas* ad vegetabilia referendas esse. Mihi vero totum hocce genus botanicis relinquendum videtur. Nec enim structurâ, nec chymicis principiis ad *Zoophytorum* ullum genus accedunt, et pleræque species etiam habitum prorsus peculiarem habent, aliquæ ad fucos potius accedentes, plurimæ confervis comparabiles, quamvis lapidescenti substantia ab iisdem et omnibus vegetabilibus distinctissimæ. Pallas *Elenchus Zoophyt.* p. 418.

He begins with observing, that they don't come near to any one genus of Zoophytes, neither in their structure nor chemical principles; that some species have a peculiar appearance, some approach to *Fucus's*, many are like *Conservas*; but that all of them are very distinct from them, and from all vegetables, on account of their lapidescent substance.

That they differ in their chemical principles from Zoophytes; for when they are burnt, they smell like vegetables: and that, according to Count Marfigli's Experiments (*Hist. Mar. p. 73.*) they neither contain a volatile salt, or animal oil.

That the pores, in their calcareous substance, are too small for polypes to inhabit them; and that the pores of *Fucus's* prove them to be as much animals as the *Corallines*, even when their pores are rendered more visible, by having the calcareous substance, that surrounds them, dissolved by an acid.

That the great Jussieu; in his diligent researches after marine productions could see no visible token of life in them.

That Mr. Meese, who has lately wrote a *Flora Frisica*, has found a *Coralline* growing upon a heath in *Frieland*; which, Dr. Pallas says, is a strong proof of their vegetable origin.

Lastly, that their fructification is so nearly analogous to those of *Fucuses* and *Conservas*, that he likewise takes that to be a proof of their belonging to the vegetable kingdom.

To proceed then.—Dr. Pallas, after telling us that *Corallines* are vegetables, says, that some of them are like *Fucuses*.

In



In this I must agree with him; because his first Coralline, which he calls *Corallina Pavonia*, is truly of that genus of plants: this most elegant *Fucus* I have particularly described and figured (Essay on Corall. p. 88. T. 33. fig. *c, d, e,*); it is well known by the name of Turkey-feather *Fucus*, and is called, in the *Species Plant.* p. 1630, *Fucus Pavonius*. What could have led Dr. Pallas into this mistake? most probably those beautiful farinaceous semi-circular stripes on it, which he must have taken for a lapidescent or calcareous substance\*, one of the most distinguishing characters of a Coralline, even according to his own description of this genus. If he had tried this farinaceous substance with an acid, he might observe, that it would not ferment; it is of the same nature with the farina that covers many plants, for instance the *Primula Auricula*, and almost all the *Lichenes foliacei* and *fruticulosi*, or Liverworts. As to their similitude to the *Conserva*, the contrary will appear, as soon as I come to give the proper definitions to both these, and the Corallines. In the same paragraph he says, that the Corallines do not come near to any genus of *Zoophytes*.

How far he is mistaken in this assertion, I will endeavour to prove from the following experiments.

Break a thin piece from the *Corallium Anglicum*, Essay on Corall. T. 27. N. 1. *c.* (*Millepora Calcareæ*, Pallas Elench. p. 265.) or of the *Corallium Lichenoides*, Essay on Corall. T. 27. N. 2. *d.*; both

\* *Quamvis lapidescenti substantia ab omnibus vegetabilibus distinctissimæ.* Pallas, Elench. 418.

which, Dr. Pallas, in his Elench. p. 265. has confounded together under the name of *Millepora Calcareæ* (but which he confesses to be animal); and when you examine them in the microscope, you will find in them both regular series of cells, as figured in Essay on Cor. Tab. 27. Fig. D. Split at the same time one of the joints of the *Corallina Officinalis* of Linnæus lengthways, and you will find the series of cells \* correspond in shape exactly with both the former; which I think proves the organization of these bodies to be the same, and consequently animal.

Besides these, compare the structure of the *Miriozoon* of Donati, Phil. Transf. Vol. XLVII. p. 107. Tab. 5. (*Millepora truncata*, Pallas Elench. p. 249.) with those of the *Corallina Rosarium*, and *Corallina incrassata*, both which I have carefully dissected and figured in Tab. XVII. Fig. 15, 20, &c. and there appears so great an affinity between their cells (and even in the opercula of the *Corallina incrassata*), that it affords us reason to conclude with great probability, that their mouths, or suckers, are the same. It cannot be amiss to mention the similitude there is between the stony-jointed Corallines, and the *Isis Hippuris*, or jointed black and white East Indian Coral, and the *Cellularia Salicornia*, Pallas Zooph. p. 61. or Bugle Coralline, Essay on Coral. T. 23. which two last are universally allowed to be animals: in all these are found the same kind of fibres that connect their joints, and exactly in the same manner.

In order to prove that these Corallines have a smell very different from vegetables, I must appeal to

\* See Tab. XVII. fig. 12 and 13.

an experiment made publickly before the Society of Arts, Commerce, &c. and which gave them a satisfactory demonstration of the great difference in nature between Corallines and vegetable substances. It happened upon the following occasion. A gentleman of Wales had sent the society a parcel of Lichen tartareus, of Linn. Ed. 2. Sp. Pl. 1608. as a proper material for dying a red colour, to answer the same purpose of that expensive article among the dyers, called Orchell, or Canary weed, which is the Lichen Roccella of Linn. Sp. Pl. 1622.

As the object was of consequence, the society was very desirous of being fully informed of the nature and appearance of this useful dye; and therefore, several curious gentlemen of the society were desired, against the next meeting, to bring some specimens of true Orchell. Accordingly some specimens were obtained from the Orchell dyers in Southwark, and laid before the society.

At the same time Dr. Maningham, a member of that society, produced before the society a specimen, in a paper with Orchell wrote upon it, from Mr. Miller of Chelsea, likewise as the true Orchell: but, upon examining it, it proved to be the *Corallina nervo tenuiori fragiliorique internodia nectente* of Sir Hans Sloane's History of Jamaica, Vol. I. Tab. 20. Fig. 4. Some disputes arising on the different appearance of the specimens, I took the liberty to inform the gentlemen present, that, having lately made some experiments on Corallines, I believed that Mr. Miller's specimen was a Coralline, or animal substance, and the Lichen Roccella, or Dyers Orchell, was a vegetable; and in order to convince the society of the

difference, I called for a lighted candle, and having first set fire to the Lichen Roccella, it yielded the same smell that burnt vegetables usually do; but when the Coralline (which was Mr. Miller's specimen) was burnt, it filled the room with such an offensive smell like that of burnt bones, or hair, that the door was obliged to be opened, to dissipate the disagreeable scent, and let in fresh air.

Another argument that Dr. Pallas offers the world of the vegetable nature of Corallines, or rather a proof of their not being of an animal nature, are Count Marfigli's Chemical experiments on the *Corallina Officinalis* (Hist. Mar. p. 73.) where he says it neither contains animal oil nor volatile salts.

But, to prevent such plausible arguments from misleading mankind, I determined to have fair and accurate experiments made on this substance. Accordingly I applied to Mr. Peter Woulfe, F. R. S. a gentleman distinguished for his great knowledge in chemistry; and in order to have the specimens fresh from the sea, I applied to a worthy member of this Society, the Right Honourable the Earl of Hillsborough, for Mr. Potts, the Secretary to the Post-Office, to procure me a sufficient quantity of the *Corallina Officinalis* from the sea-coast near Harwich: this parcel, about two months ago, I sent to Mr. Woulfe; and in answer have received the following letter, with an account of his experiments made on it.

Clerkenwell.

Clerkenwell, May 5, 1767.

S I R,

**I** TOOK twelve ounces troy of the Corallinae Officinalis (which you sent me) picked clean from every extraneous substance, and put it into a clean stone-coated retort; the retort was set in a reverberatory furnace, and an adapter and quilled receiver luted to it: the fire was very gentle for the first eight hours; in which time, half an ounce and eighteen grains of a transparent and almost colourless liquor came over, which was set aside for examination. The fire was then increased, and in six hours time there were distilled two drams and three grains of a turbid liquor, which had some appearance of oiliness on its surface; this was likewise set a-part to be examined. The fire was then increased for six hours longer, and during the last two hours the retort was quite red hot all over, which ended the distillation. In this third and last process the portion of liquor that came over was more turbid than the second, and some of it from the redundancy of its volatile alkaly was crystallized; it also contained rather more than a dram of light empyreumatic oil, very much resembling the smell of hartshorn; in the recipient there was also some crystals of a volatile alkali. The whole of this last product weighed three drams and an half. The caput mortuum was quite black, and weighed ten ounces, one dram, and one scruple; so that there was a loss of four drams and forty-nine grains out of the twelve ounces of Coralline.

The first liquor that distilled slightly effervesced with spirit of salt, and changed syrup of violets green, certain proofs of a volatile alkali.

The second and third portion effervesced strongly with spirit of salt, as did also the volatile salt that came over into the receiver, evident marks of its being a concentrated alkali.

Here I must observe, that had this distillation been conducted in a hurry, there would have been no concrete volatile alkali; for then this would have been confounded and dissolved in the first liquor that came over.

Had there been a sufficient quantity of this Coraline, I should first have proposed to have taken off the calcareous substance, by an acid menstruum, and afterwards washed the membranaceous part so clean from the acid, as not to change the syrup of violets red.

Then the distillation of this part alone would have afforded a much larger proportion of empyreumatic oil, and volatile alkali, and but a very small quantity of caput mortuum.

If you think these experiments of any use, you have my free leave to lay them before the Royal Society.

I am, Sir, yours, &c.

To John Ellis, Esq;  
in Grays Inn.

Peter Woulfe.

Doctor Pallas proceeds to prove that Corallines cannot be animals, as the \* pores of their calcareous

\* Pori autem calcaræ substantiæ ita sunt minuti, ut polypi in his hospitari nequeant. Pall. Elench. p. 419.

substances

substances are too minute for any polypes to harbour in. These words of the Doctor's seem to imply, as if the Coralline substances were only habitations for detached polypes, and not part of the animals themselves. How this affair stands, I hope to have clearly demonstrated long before this, for I have plainly seen, and endeavoured to shew mankind, that the softer and harder parts of zoophytes are so closely connected with one another, that they cannot separately exist; and therefore have not hesitated to call them constituent parts of the same body, and that the polype-like suckers are so many mouths belonging thereto.

Now, for the smallness of the pores, which the Doctor has mentioned here (among the Corallines) to be a contradiction to animal life; he certainly has forgot one circumstance, when he introduces the *Corallium pumilum album* (Essay Cor. T. 27. f. c.) or his *Millepora calcarea* (Pall. Elench. p. 265.) as an animal, which is, that he there says, it has absolutely no pores at all†.

As there can be no doubt, but every part of what is called Coralline is necessary to make out such an animal, or being, it will be very difficult, if not almost impossible, to determine the proportion there ought to be between softer and harder parts; and therefore it cannot be thought unreasonable to say, that in some of this tribe the stony parts are by much the greater part of the whole, especially as Doctor Pallas's objection can be only against the crust, or lapidescent part, as the inside of many of them is far from being hard, being

† Pori omnino nulli. Pall. Elench. p. 266.

exactly like a *Sertularia*, so that I do not know if it would not be a good definition to one well acquainted with that tribe to say, a *Coralline* is a *Sertularia* covered with a stony or calcareous crust; if the mouths should happen to be very small, their number may make up that deficiency. We see in the greatest number of *Corallines* their surface full of holes; we saw the same in *Escharas* and *Milleporas* thirty years ago; since that time magnifying glasses have been improved, so as to shew us, that they are all orifices, for polype-like suckers; why should not we now admit that glasses may be still more improved, so as even to make us able to see what may be the intention and use of these minute orifices, which according to all rules of reasoning, we must suppose to approach in nature to them they are most alike. From this extreme minuteness then of the pores of these *Millepora*, confessed to be zoophytes, as well as those of *Corallina officinalis* as before mentioned, it is no great matter of surprize, that Doctor Jussieu could not perceive any animal life in the *Corallines*, nor Doctor Schlosser in the *Millepora calcarea*. As these experiments ought to be attended with many convenient coinciding circumstances that do not often happen to persons who only go to the sea-side, perhaps for a few days, or hours, so that it is unreasonable to conclude, because they have been unsuccessful, that more accurate observers may not be more fortunate at another time.

I believe I shall be justified in this, by many essays that have been made, by persons of judgment, to observe the polype-like suckers in many, even of the *Sertulariæ*, which they have several times attempted

in



in vain; I must own it has often happened to me in many species, and yet I have not the least doubt of their being true *Sertulariæ* from the similarity there is in their habit and form to others of the same genus; and of this fact I am sure Doctor Pallas is fully convinced.

Another argument made use of by Doctor Pallas, to overthrow the animal existence of Corallines, is taken from Mr. Meese's assertion, that he had found on Bergummer Heath in Friesland, a substance of the same nature with the Corallines. Meese, in his *Flora Frisica*, p. 75. calls it a *Lichen*; but Doctor Pallas has ventured in his *Elench.* p. 427. to rank it among the Corallines, under the name of *Corallina terrestris* \*. In this Doctor Pallas is in the right, as I have had an opportunity of examining a small specimen, that my worthy and learned friend Doctor Schloffer of Amsterdam was so kind to procure me: but how such a nice and accurate philosopher as Doctor Pallas could let it escape him to consider the nature and quality of this subject, and how much it differs from any thing else growing on the land, is a thing that surprises me. It only being mentioned by Mr. Meese, as found on Bergummer Heath, ought not to have satisfied him so far, as to declare a body with a calcareous crust to be a land production, when no such thing in the whole vegetable kingdom has ever been found; it has always been thought quite the contrary, that a stony or hard substance of that nature, could not be produced, but from an animal, and chiefly those that live under water †.

\* See the figure of it in Tab. XVII. fig. 28.

† 'Tis worthy of our notice how easily this ingenious Natural Historian reconciles it to himself, that this inhabitant of the

This should certainly have made him minutely inquire in what manner it was found, if buried under moss, loose on the ground, or perhaps near some of the canals, which communicate with the sea. Many accidents might have brought it thither, which is more probable than to imagine nature to go out of her usual track.

It is not improbable that that part of Holland has been overflowed by the sea, and this production left there when the water subsided, or blown there by a storm, which I beg leave to believe till I am better informed. I do not in the least doubt of Mr. Meese's veracity; but as that gentleman was more intent on discovering vegetables than animals, and thinking this very like a dry *Lichen fruticulosus*, he did not scruple to believe it to be one of that tribe; and therefore, perhaps, neglected to observe all those circumstances, that we now wish to be informed of.

The irregular pedunculated figures or fructifications (as Doctor Pallas pleases to call what is represented in Tab. XVII. fig. 29.) seem to be rather a defect in the growth of the ramifications, especially as they differ from one another in shape, and some of them appear beginning to form other branches.

In fig. *a* the whole consists of two opposite curled processes, with a small cavity between them at the top; this cavity is filled up at fig. *b*. so that the top becomes rounded; in fig. *c. c.* there seems to be a beginning of a continuation lengthways; and in fig. *d.* it is still more plain the beginning of a branch.

sea can grow on dry land. See Pallas Zoophyt. p. 427. Nec magis miror Corallinam in sicco crescentem, quam Lichenum cum Fucis summam analogiam.

If the inside of these processes had been hollow, and the outside of a regular figure, I should not have hesitated to consider them to be the ovaries of the Coralline; but as they are solid, and of the same structure with the rest of Corallines, I shall rather call them defective branches.

Doctor Pallas's last argument to prove that Corallines are vegetables is, that the nodules, or tubercles, which he has observed in Corallines, contain little seeds subanalogous, or somewhat resembling those we find in the fructification of the Fucus's and Confervas.

If this method of reasoning should hold good, what will become of the Cellularias, Sertularias, and Millepora calcarea & agariciformis, with many other zoophytes, that have such roundish ovaries; they must be recalled to the vegetable kingdom, notwithstanding all doubt about their being living animals has long been laid aside.

I come now to his ingenious friend Doctor Baster, who carries this matter still farther, and says positively, in Phil. Trans. Vol. LII. p. 111. that the Corallines are true Confervas; and in his Opuscula Subseciva, Vol. I. Tab. I. fig. 3. A. and B. he refers us to the figure of the Corallina rubens in seed; which, he says, is a true Conferva; but the figure is so bad, that I am persuaded nobody can find out what he means to represent by it.

I shall therefore conclude this letter, with recommending to these ingenious gentlemen, to analyse these bodies chemically, and with care; and likewise to view them with the same attention, that I have done, in the microscope; if so, I am

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perswaded they will be of our opinion. I must defer the sequel of what I intended to another day, which was to give you an account of the discoveries I have made in the fructification of the *Confervas*; these, I flatter myself, will fully convince Doctor Baister of the great difference between these two bodies, and that they belong to two different kingdoms of Nature.

I am,

S I R,

Your most obedient Servant,

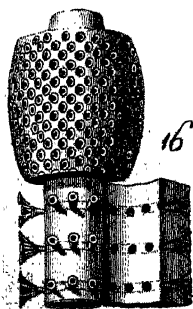
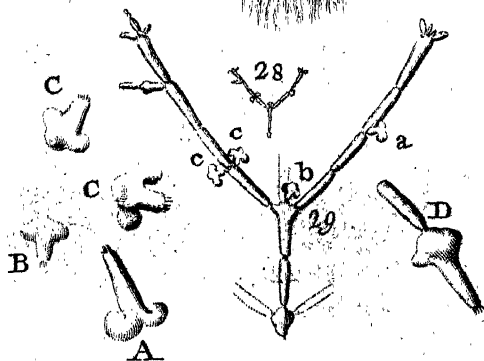
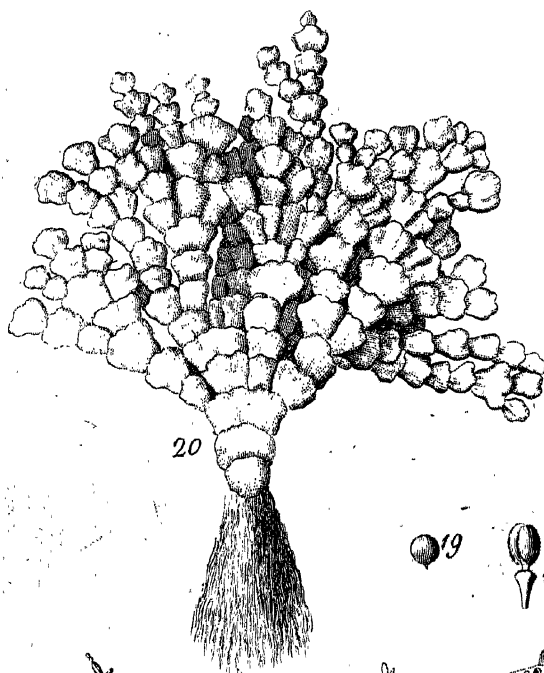
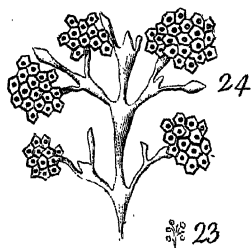
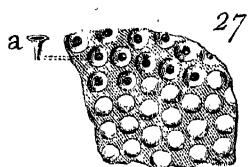
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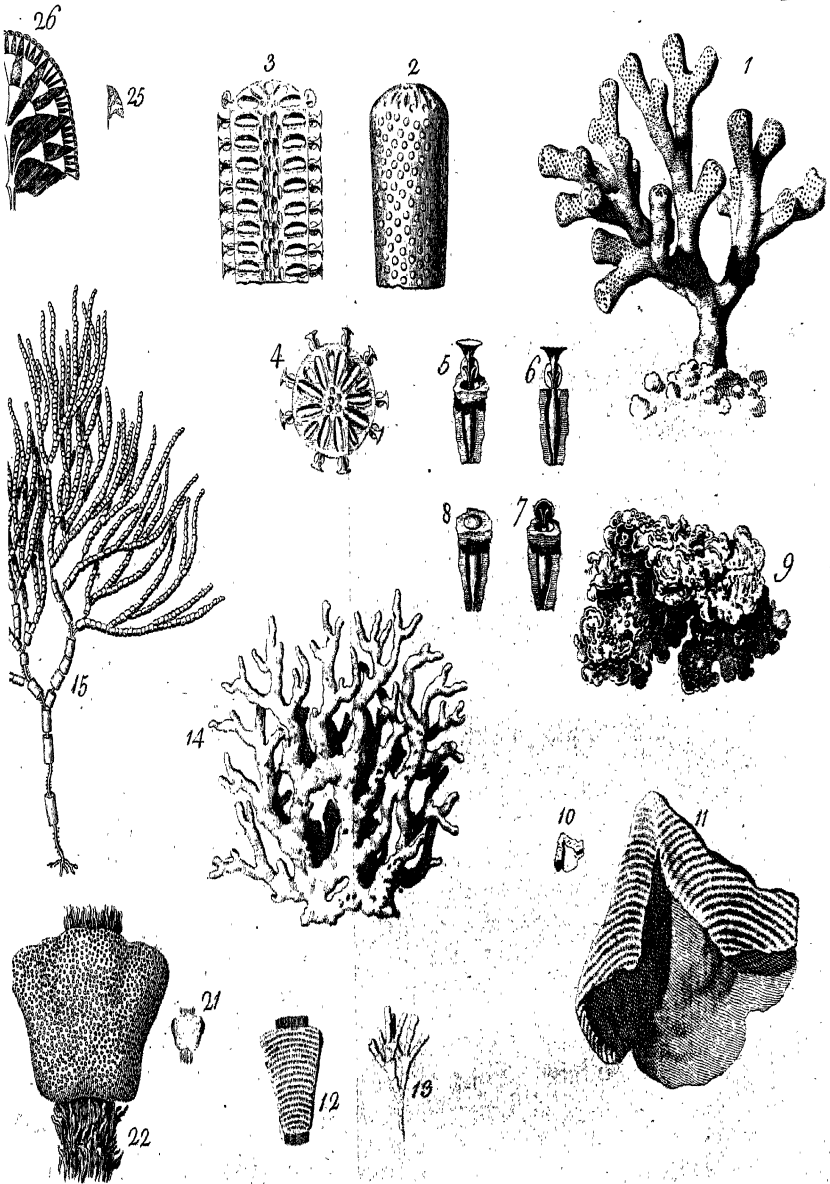
John Ellis.

### The Description of Plate XVII.

- Fig. 1. The *Miriozoon* of Donati, or *Millepora truncata* of Pallas.
2. The end of a branch magnified, to shew the situation of the pores.
3. The same cut perpendicularly through, to shew the Trumpet-like suckers in their cells connected with the middle tubes.
4. The horizontal section of the same, with the suckers extended.
5. The magnified drawing of one of the suckers, with its cell and operculum.
6. The











- Fig. 6. The oblique view of the opening of the cell with the sucker and operculum.
7. The cell with the operculum open.
8. The cell covered with its operculum.
9. The *Corallium Lichenoides* of Ellis's Corallines, with ovaries upon it.
10. The natural and magnified size of a piece  
and 11. of this Coral, to shew the arrangement of the inside of the cells, which are just the same as in the following.
12. The order of the cells, in a joint of the *Corallina Officinalis*, to shew the great affinity between them.
13. The natural size of a small piece of the *Corallina Officinalis*.
14. The milk-white *Millepora calcarea*, from the Mediterranean, where, though the pores are not visible on the outside, the arrangement of the cells in the inside are the same with the *Corallium Lichenoides*, and *Corallina Officinalis*.
15. The *Corallina Rosarium*, or White-bead band-string of Sloan's Hist. of Jamaica, Tab. XX. fig. 3.
16. Two joints magnified, one to shew the situation and figure of the pores, and the other to shew how the suckers pass from the middle cartilaginous tube through the calcareous covering to the surface.

- Fig. 17. Shews four of the suckers, and the ovary between them, magnified highly.
18. The Ovary.
19. One of the eggs taken out of the ovary.
20. The *Corallina incrassata*, from the West-Indies.
21. One of the joints of its natural size.
22. The same magnified a little, to shew its pores in its calcareous surface.
23. Part of the inside tubes of the joint, of their natural size.
24. The same magnified, to shew the openings of the cells on the surface, connected together.
25. A perpendicular section of half of one of these joints.
26. The same magnified, to shew the figure of the vessels leading to the suckers in the calcareous surface.
27. A piece of the calcareous surface highly magnified, to shew some of the pores open, and others covered with their convex opercula; letter *a* shews the figure of one of the trumpet-shaped suckers highly magnified.
28. A small branch of Meese's Coralline supposed to grow on a heath, called by Dr. Pallas *Corallina terrestris*.
29. The same magnified, to shew the disposition and figures of its supposed fructification at *a*, *b*, *c* *c*. and *d*. which are higher magnified at A. B. C. C. and D. to shew how unlike they are to fructifications.

## S E Q U E L.

Title read December 17, 1767.

Read Jan. 14, 1768. **I** COME now to answer Doctor Bafter, who asserts positively, in his memoir published in the Transactions of the Royal Society, Vol. LII. p. 111, that all the Corallines, which you and I have described, are plants of the genus of *Conserva*.

In order to explain myself, it will be necessary to let him know what I mean by a *Conserva*, and what I would be understood by a Coralline, according to your system.

By a *Conserva* I mean a plant with jointed filaments, either single or branched, bearing fruit, which are disposed in different ways: in Latin, thus,

*Conserva est planta, cui sunt filamenta articulata, vel simplicia vel ramosa, fructificationes vario modo dispositæ habentia.*

By a Coralline I mean an animal growing in the form of a plant, whose stem is fixed to other bodies. The stem is composed of capillary tubes, whose extremities,

tremities pass through a calcareous crust, and open into pores on the surface. The branches are often jointed, and always sub-divided into smaller branches, which are either loose and unconnected, or joined, as if they were glued together: in Latin, thus,

*Corallina est animal crescens habitu plantæ.*

*Stirps fixa, e tubis capillaribus per crustam calcaream porosam sese exerentibus composita.*

*Rami sæpe articulati, semper ramulosi, vel divaricati liberi, vel conglutinati et connexi.*

This difference then will evidently appear by putting each kind into an acid liquor. The Coralline will immediately discover the nature of its \* calcareous surface, by a strong fermentation; when the Conferva will not appear in the least affected. This acid liquor will likewise soon dissolve the calcareous substance in the Coralline, by which means the minute vessels that lead to the pores on the surface will become visible; whereas the Conferva will unalterably remain the same, and be rather preserved than corroded by the acid.

When Doctor Pallas, who supports the opinion of Doctor Baister, comes to the chemical analysis of the Corallines, he tells us † that he had not time nor opportunity to try them; but depends on the report of other authors.

\* Lin. Syst. Nat. Ed. 12. p. 1304. “ Corallinas ad regnum animale pertinere ex substantia earum calcarea constat, cum omnem calcem animalium esse productum verissimum sit.”

† Pallas Zooph. p. 418. “ Temporis angustia et opportunitas impediverit ne in Corallarum naturam accuratius igne inquirerem.”

This dependance on the authority of others, to overturn what I think we have established with very strong evidence, will, I am in hopes, convince him of the propriety of that well-chosen motto of the Royal Society. “Nullius in verba;” which I find he has adopted as the common seal of his epistles to his literary correspondents: and he will now have a further opportunity of \* complimenting Doctor Baster on making a second apology for what he has advanced against me in the *Phil. Trans.* Vol. LII. p. 111. by shewing him, that they have both been mistaken in blending two very different genera of the animal and vegetable kingdoms of nature together.

To make this difference appear still more evident, I come now to lay before you a new scene of nature; which an accurate examination into the fructification, as well as the articulations, of some of the *Confervas*, afforded me. Indeed the minuteness of these objects would scarce seem worth while to examine into so critically, if my reputation had not engaged me to shew the wide difference between them and *Corallines*. This, joined to some remarkable discoveries, which I made in the year 1754 on the coast of *Suffex* (in company with Mr. G. D. Ehret, F. R. S.) in the fructification of this class of plants, which

\* *Pallas Zooph.* p. 20. “*Candidissimus Basterus, qui hucusque contra Ellisium reliquosque prioris sententiæ patronos steterat, alterius evidentiae victas dedit manus, et gloriosissimo exemplo, repudiata priori sua opinione, veram theoriam acriter defendere coepit.*”

before that time were esteemed by \* botanical writers to have no fructification at all, has induced me to lay a few specimens of them with their magnified drawings before the Royal Society.

In examining these plants I was amazed to find two species of them evidently of your class of Diœcia; that is, male parts of fructification on one, and female on the other.

The first of these is the *Conferva polymorpha*, where in Tab. XVIII. at fig. *a.* is represented a very small branch of the female in its natural size, and at fig. *A.* the same is magnified: in the transparent capsules of this specimen, we can easily discover the seed as it lies expanded in a watch-glass in water. Letter *b.* represents the natural size of a small branch of the male. Letter *B.* the same branch magnified, shewing its amentaceous flowers, or catkins, with its minute male seed in spikes. *B i.* shews one of them highly magnified.

The other *Conferva* is the *Plumosa*, and is one of our most elegant sub-marine plants. Fig. *c.* represents the natural size of a minute sprig of the female. At fig. *C.* the same is magnified, where the seeds appear in their capsules. The fig. *d.* shews the natural size of a sprig of the male *Conferva plumosa*; and fig. *D.* the same sprig magnified, shewing the spikes of male seed.

\* Ray, Synop. Ed. 3. p. 57. "*Conferva. est. Musci genus sterile et capitulis floridis destitutum, immo nec peltis & tuberculis, quæ horum loco aliqui gerunt, donatum, ex meris foliis teretibus et uniformibus seu mavis cauliculis, in tenuia capillamenta divisis, constans.*"

The next is the *Conserva flosculosa*, and is represented at fig. *e.* in a branch of the natural size. Fig. *E.* is the same magnified. This is one of those remarkable *Conservas* that has footstalks to its flowers or fructification. It appears to have fruit like a strawberry, or raspberry, surrounded by a leafy calyx.

This was found on the sea-coast, near Yarmouth in Norfolk, by my worthy friend George Whatley, Esquire, in the year 1764. When it was fresh, it was of a most vivid carmine colour. The other with flowers, at fig. *f.* is the *Conserva geniculata*. Fig. *F.* shews the same branch more distinctly, being magnified with flowers surrounding the joints; this, with one which I have called in my catalogue of *Conservas*, *Conserva florifera*, I discovered in the year 1754 near Brighthelmston in Suffex, when Mr. Ehret was so kind as to make drawings of them while recent. The colour of this, when fresh, is a fine scarlet.

The *Conserva plumula*, at fig. *g.* is one of the smallest of the tribe, but most elegantly feathered; it is of a pale red colour. The same is magnified at fig. *G.* which shews the order that the fruit and branches are disposed in. *G 1* shews the fruit or seeds, which are of a red colour, surrounded by a clear gelatinous pulp.

The *Conserva* at fig. *b.* I have called *Ciliata*, from the circle of small fibres at the top of each joint. The magnified drawing at fig. *H.* shews these fibres like a crown on each joint. This was inserted here to shew, with the rest, some of the infinite variety of beautiful forms, which the great Author of nature has impressed even upon one of the lowest classes of the vegetable tribe.

Before I conclude, I must observe ;

That as Doctor Pallas has likewise introduced among his arguments, that the fruit of the Fucus's are subanalogous to those of the Corallines, I could introduce an infinite variety to shew the great difference there is between them ; but this part of natural history, too long neglected, requires a volume by itself, to shew the amazing variety of vegetables, that lie hid from us in the great deep ; I may make some observations on them the subject of a future letter, especially as many of them are of the class of Dioccia, as well as those which I have already shewn in the Conervas ; which I believe will be new to the botanists.

I am,

Dear Sir,

Your most obedient servant,

John Ellis.

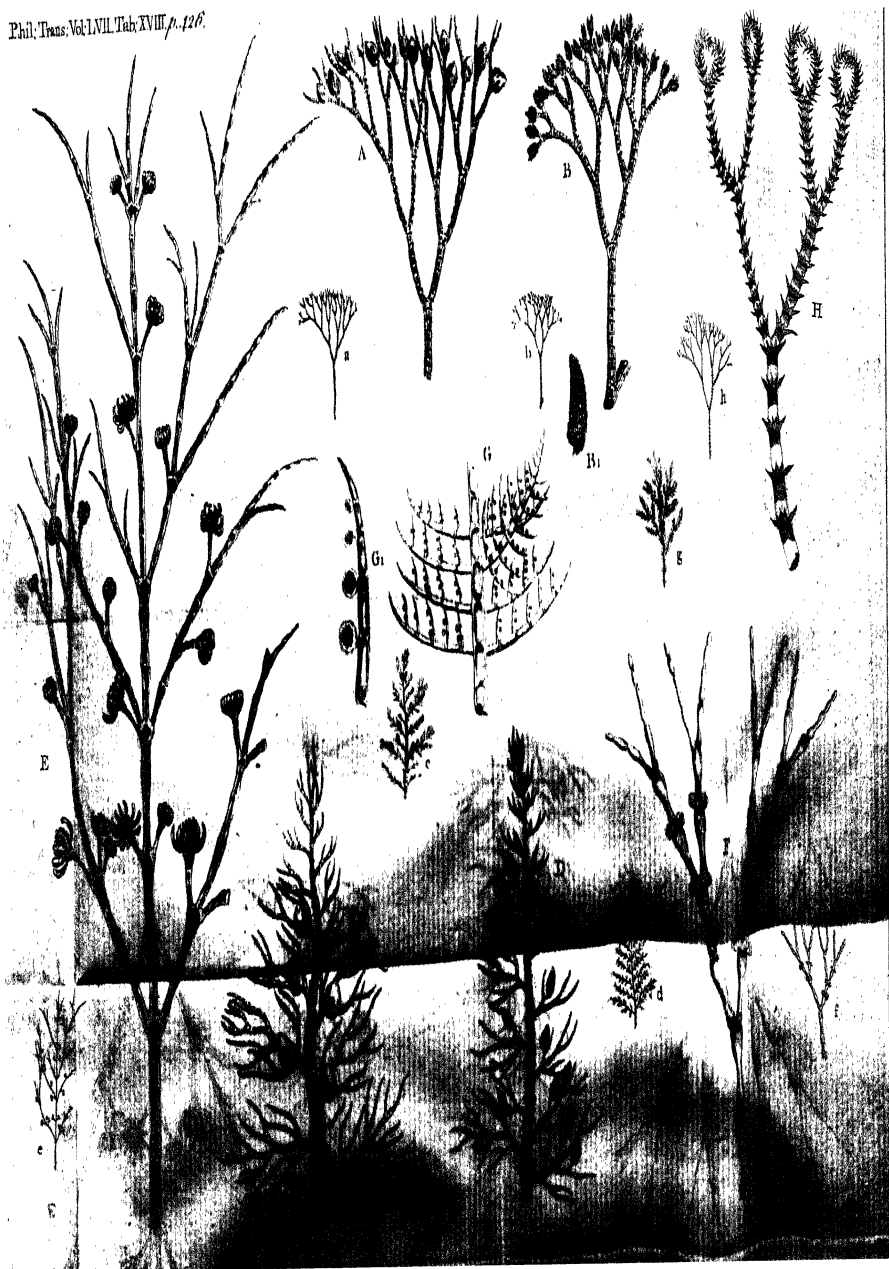
### The Description of Plate XVIII.

- Fig. *a.* The female *Conserva polymorpha*.  
*A.* The same magnified, to shew the seed in the Captules.  
*b.* The male *Conserva polymorpha*.  
*B.* The same magnified, with its male flowers.  
*B x.* One of the catkins, or male flowers, highly magnified.  
*c.* The female *Conserva plumosa*.

The









- Fig. C. The same magnified, to shew its fructification.
- d. The male *Conferva plumosa*.
- D. The same magnified, shewing its catkins, or male flowers.
- c. *Conferva flosculosa*.
- E. The same magnified, shewing its pedunculated flowers, or fruit, with their polypetalous cups.
- f. *Conferva gonculata*.
- F. The same magnified, to shew its flowers surrounding the joints.
- g. *Conferva plumula*.
- G. Part of it magnified, to shew the disposition of its branches.
- G. a. ~~Some of the fruit~~ highly magnified, to shew its seeds, surrounded by a clear viscid pulp.
- b. *Conferva ciliata*.
- " H. The same magnified, to shew the little coronets on the joints.

XLI. *An Account of the Actinia Sociata, or Clustered Animal-flower, lately found on the Sea-Coasts of the new-ceded Islands : In a Letter from John Ellis, Esquire, F. R. S. to the Right Honourable the Earl of Hillsborough, F. R. S.*

My Lord,

Read Nov. 12,  
1767.

**A**MONG the many curious marine animals, which your Lordship has received from the new-ceded islands in the West-Indies, there is one most uncommonly rare : this is of great consequence to natural history, as it seems to bring together two remarkable genera in the system of nature, which Professor Linnæus had removed far from each other.

The one is the Actinia or Animal flower, the other the Hydra or Fresh-water polype.

The Actinia, called by old authors, as Aldrovandus, Johnston, &c. *Urtica marina*, from its supposed property of stinging, is now more properly called by some late English authors the Animal flower. This name seems well adapted to it, for the claws, or tentacles, being disposed in regular circles, and tinged with a variety of bright lively colours, very nearly represent the beautiful petals of some of our most elegantly fringed and radiated flowers, such as the Carnation, Marygold, and Anemone. As there  
are

are great variety of species of this animal, so these species differ from each other in their form. The bodies of some of them are hemispherical, others cylindrical, and others shaped like a fig. Their substance likewise differs; for some are stiff and gelatinous, others fleshy and muscular; but they are all capable of altering their shape, when they extend their bodies and claws in search of their food. We find them on our rocky coasts at low water, fixed in the shallows to some solid substance, by a broad base like a sucker; but they can shift their situation, though their movement is very slow.

They have only one opening, which is in the center of the uppermost part of the animal; round this are placed rows of fleshy claws; this opening is the mouth of the animal, and is capable of great extension: it is amazing to see what large shell fish some of them can swallow, such as muscles, crabs, &c. when it has sucked out the fish, it throws back the shells through the same passage. Through this opening is ~~the same passage~~ already furnished, when they begin to extend in search of food.

They are found all round the coasts of England; but the coasts of Sussex and Cornwall furnish us with the greatest varieties of them. The islands in the West Indies are likewise remarkable for many kinds of them, as appear from the different sorts sent to your Lordship by Mr. Greg.

Dennis Gaster, F. R. S. who has described four species of the *Ascidia* in the Phil. Trans. \* says

\* Vide Phil. Trans. Vol. LII. p. 75. Tab. 4. figs. 4, and 5; the animal in fig. 3, in the same Plate is sucked in

they have the remarkable property of renewing their claws when they are cut off; and ranks them, perhaps very properly, under the genus of *Hydra* of Linnæus, or Fresh-water polype: which I shall now give a short description of, that we may judge how near your Lordship's new animal approaches to both of these.

The *Hydra*, or Fresh-water polype, is that extraordinary animal so well known to the curious, from the discoveries of Mr. Abraham Trembley, F. R. S. in its re-production after it had been cut into pieces. When it is extended, it is of a worm-shaped figure, and of the same tender substance with the horns of a common snail.

It adheres by one end like a sucker to water plants and other substances: the other end, which is the head, is surrounded by many arms or feelers placed like rays round a center: this center is its mouth, and with these arms, which are capable of great extension, it seizes small worms and water insects, and brings them to its mouth; often swallowing bodies larger than itself: when the food is digested in the stomach, it returns the remains of the animals it feeds on, through its mouth again, having no other visible passage from its body.

Their manner of multiplying is from eggs, which they produce in autumn\*; but the most common is from their sides, in which there first appear small knobs, or papillæ; as these increase in length, little

this genus by Doctor Pallas, as well as Doctor Gaertner, but very improperly, as it has many feet, and a passage through its body. Doctor Linnæus calls it *Holothuria*.

\* See Pallas, *Zoophyt.* p. 28.

fibres

fibres are seen rising out of the circumference of their heads, which they soon use to procure food. When they are thus arrived at a mature state, they send forth other young ones from their sides: so that though many of them soon fall off, and provide for themselves, yet the animal frequently branches out into a numerous offspring, growing out of one common parent, each of which not only procures nourishment for itself, but for the whole family.

I come now to your Lordship's new animal; and, for the Satisfaction of the Royal Society, lay before them one of your Lordship's specimens preserved in spirits, with a dissection of one of them, to shew its internal structure, together with three species of Actinia, or Animal flowers, sent to your Lordship from the new-ceded islands.

This compound animal, which is of a tender fleshy substance, consists of many tubular bodies, swelling gently towards the upper part, and ending like a bulb, or very small onion; on the top of each is its mouth, surrounded by one or two rows of tentacles, or claws, which when contracted look like circles of beads.

The lower part of all these bodies have a communication with a firm fleshy wrinkled tube, which sticks fast to the rocks, and sends forth other fleshy tubes, which creep along them in various directions. These are full of different sizes of these remarkable animals, which rise up irregularly in groupes near to one another.

This adhering tube, that secures them fast to the rock, or shelly bottom, is worthy of our notice. The knobs that we observe, are formed in several parts



parts of it, by its insinuating itself into the inequalities of the coral rock, or by grasping pieces of shells, part of which still remain in it, with the fleshy substance grown over them.

This shews us the instinct of nature, that directs these animals to preserve themselves from the violence of the waves, not unlike the anchoring of muscles, by their fine silken filaments, that end in suckers; or rather like the shelly bases of the Serpula, or Worm-shell, the Tree Oyster, and the Slipper Barnicle, &c. whose bases conform to the shape of whatever substance they fix themselves to, grasping it fast with their testaceous claws, to withstand the fury of a storm.

When we view the inside of this animal dissected lengthways, we find a little tube like a gullet leading from the mouth to the stomach, from whence there rise eight wrinkled small guts, in a circular order, with a yellowish soft substance in them; these bend over in the form of arches towards the lower part of the bulb, from whence they may be traced downwards, to the narrow part of the upright tube, till they come to the fleshy adhering tube, where some of them may be perceived entering into a papilla, or the beginning of an animal of the like kind, most probably to convey it nourishment, till it is provided with claws: the remaining part of these slender guts are continued on in the fleshy tube, without doubt for the same purpose of producing and supporting more young ones from the same common parent.

The many longitudinal fibres, that we discover lying parallel to each other, on the inside of the  
semi-

semi-transparent skin, are all inserted in the several claws round the animal's mouth, and are plainly the tendons of the muscles, for moving and directing the claws, at the will of the animal; these may be likewise traced down to the adhering tube.

As this specimen has been preserved in spirits, the colour of the animal when living cannot certainly be known; it is at present of a pale yellowish brown.

With regard to its name, it may be called *Actinia sociata*, or the Cluster animal flower.

Among the critics, my Lord, I am aware of this; that it may be said, that an animal compounded of many animals has not a very philosophical sound. But it is well known to those, who understand the nature of zoophytes; that there are many kinds of these animals, as well such as swim about freely, as such as are fixt to rocks and shells in the sea, that have a great many mouths in the form of polypes, and yet are but single animals; such as the great variety of *Pennatulas*, or Sea pens, among those that swim about, and most of the *Sertularias*, *Gorgonias*, with many others, among those that are fixt. Yet this new animal of your Lordship's differs very much from the generality of these. I think I may compare it, to speak in the style of those who maintain that zoophytes vegetate, to a timber tree, that sends out at a distance round it many suckers from its roots, which suckers coming in time to be trees, these may and will, with propriety, be reckoned so many distinct trees, though connected at their roots with the parent tree, and that without any absurdity.

Left any doubt should still arise in this abstruse part of the operations of nature, it may be proper that I should explain myself further, by shewing that there are a great many zoophytes, which were formerly called Corallines, now Sertularias and Cellularias, that from a creeping adhering tube send up several single animals, others send up several branched animals. To give an instance or two of each, I shall mention the *Sertularia uniflora*, or Single bell-shaped Coralline (see the Essay on Corallines, Pl. XIV. fig. A and B) and the *Cellularia anguina*, or Snake's head coralline (see the same Essay, Pl. XXII. fig. C) both which, like our *Aëtinia sociata*, send up distinct animals with one mouth each.

Whereas the *Sertularia pumila*, or Sea oak coralline (see Essay on Coralline, Pl. V. fig. A) and the *Cellularia bursaria*, or Shepherd purse coralline (see the same Essay, Pl. XX. fig. A) send out animals, in the form of spikes or branches, that have many mouths from their own creeping and adhering tubes; and yet both those with one mouth to each, and these with many, I esteem as so many distinct animals, notwithstanding their being connected by an adhering tube, as I have said in the instance of the tree and its suckers.

To conclude, my Lord, the importance of the discovery of this new animal to natural history is this, that it clears up that much-disputed point, which is, that the extension or increase of the substance of these zoophytes is of an animal, and not of a vegetable growth (as some late authors would have us think) by thus making the fact more clear and evident to our senses.

For the poetical descriptions of some late systematical authors have tended rather to confuse than explain these matters to our ideas ; for instance, they call these bodies, that rise up like a spike with many mouths, a vegetating stem, and their mouths, which are formed like so many polypes, flowers ; though with these supposed flowers, they evidently seize their food, by stretching out their claws (which they call the petals) to convey it to their mouths, that are in the center of each, to swallow it, digest it, and return the non-nutritive parts back again by the same way. Can this then be called a vegetative life ?

But happily this animal of your Lordship's is large enough for dissection ; and in that state discovers to us, not only muscles and tendons, but a stomach to digest, and intestines to secrete, proper nourishment for the support and increase of itself and its progeny ; which I am persuaded is the strongest proof that has yet appeared to convince the learned world, that zoophytes are true animals, and in no part vegetable ; and that the Royal Society are highly obliged to your Lordship for this most valuable acquisition in natural history, as well as he who has the honour to be,

My Lord,

Your Lordship's most devoted,

and much obliged humble servant

Gray's-Inn,  
Aug. 17, 1767.

John Ellis.

The Description of Plate XIX.

- Fig. 1. The *Actinia sociata*, or Clustered animal flower, with its radical tube adhering to a rock. (*a*) One of the animals stretching out its claws.
2. A perpendicular dissection of one of these bodies, to shew the gullet, intestines, stomach, and fibres, or tendons, that move the claws. (*a*) A young one arising out of the adhering tube.
3. The *Actinia aster*, or Sea star flower, from the new ceded Islands.
4. The *Actinia anemone*, or Sea anemone, from the same place.
5. The under part of the same, by which it adheres to rocks.
6. The *Actinia helianthus*, or Sea sun-flower, from the same place.
7. The under part of the same.
8. The *Actinia dianthus*, or Sea carnation, from the rocks at Hastings in Suffex: this animal adheres by its tail, or sucker, to the under part of the projecting rocks, opposite to the town; and, when the tide is out, has the appearance of a long white fig: this is the form of it when it is put into a glass of sea-water. It is introduced here as a new variety of this animal, not yet described.



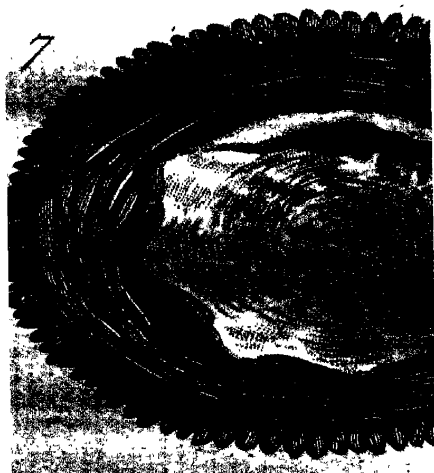
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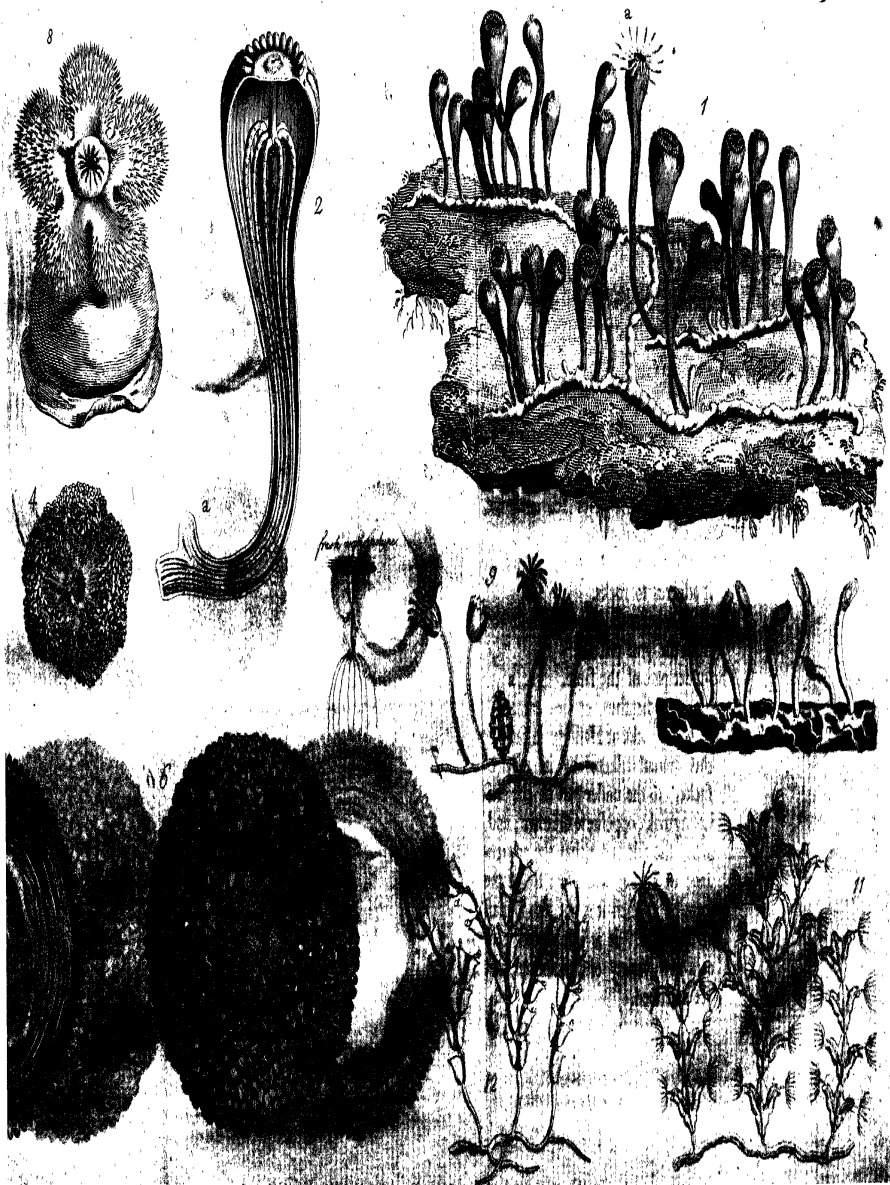


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- Fig. 9. The *Sertularia uniflora*, or Single bell-shaped coralline magnified. (a) One of its ovaries.
10. The *Cellularia anguina*, or Snake's-head coralline, magnified.
11. The *Sertularia pumila*, or Sea-oak coralline, magnified. (a) One of its ovaries.
12. The *Cellularia bursaria*, or Shepherd's purse coralline, magnified.

Received October 10, 1767.

XLII. *A Letter from Edward Wortley Montagu, Esquire, F. R. S. to William Watfon, M. D. F. R. S. containing some new Observations on what is called Pompey's Pillar, in Egypt.*

S I R,

Read Nov. 19, 1767. **I** HERE send you a few lines, which I believe will appear extraordinary, as every traveller that has been at Alexandria has mentioned the famous pillar of Oriental Granite, which is about a mile without the walls of that city, as erected, either by Pompey, or to the honour of Pompey. As I differ in opinion from them all, and think this famous pillar was erected to the honour of Vespasian, you certainly will expect to hear on what foundation I found so extraordinary a conjecture, as so new a one may appear to you.

By

			F. In.	
By my mens. the capital of the pillar is			9 7	
The Shaft	_____	_____	66 1	British Measure.
The Base	_____	_____	5 9	
The Pedestal	_____	_____	10 5	
			<hr/>	
Height from the Ground		_____	92 0	
Its Diameter	_____	_____	9 1	

As soon as I saw this surprizing pillar, I was convinced that, if it had been erected in Pompey's time, Strabo, or some of the ancients, would have mentioned it: I therefore determined to examine it narrowly. I perceived too that the pedestal was of a bad and weak masonry, composed of small and great stones of different sorts, and absolutely unable to sustain so great a weight; I therefore easily concluded such pedestal not originally belonging to the pillar. I attempted to get out a stone, which I did without trouble, and discovered the pedestal to be hollow. After some time, I mean during the course of many days, I made an opening wide enough to enter it; when within it, you will judge how much I was surprized to find this prodigious mass of granite, stood, as on a pivot, on a reversed obelisk, as I then believed it was, only five feet square; curious to know the length of the obelisk, I began to move the earth on one of its sides, but my surprize increased much when I found, after moving a few inches of the soil, that the obelisk was not entire, this pivot being only four feet and one inch thick. It is seated on a rock; the stone is of an extreme hardness,

ness, and almost a petrification or rather conglutination of many different stones, but all vitrescent. I never met with any stone of this kind any where, except with one small piece on the plain of the Mommies; I broke a piece of it, which Lord Bute has; a small piece too of the pillar was sent, that gentlemen may be convinced it is of red granite, and not a composition as some have imagined.

This part of obelisk is covered with hieroglyphicks, which are reversed, a plain proof the pillar was not erected whilst they were held sacred characters.

Convinced, therefore, that it was not of the antiquity one would suppose it, from being called of Pompey, I visited it several times to see if it might not be possible to find out something that would give room for a reasonable conjecture, in honour of whom, or at what time, it was erected. From the inscription I could discover nothing; it is on the west face of the base, but so much injured by time, and I may say too by malice, for the mark of an instrument are plainly discovered effacing it, that one can but imperfectly make out some Greek characters, so imperfectly indeed that no one word can be found.

At length, observing that the cement, or mortar, which closes the small separation of the shaft from the base, was quite destroyed in one part, I was curious to see if any thing was made use of within, to fasten or tie the shaft to the base; I saw there was: being desirous to know if it was lead, and if so, if it was not of that pure, and of which we still meet with some few medals; I endeavoured with a pretty large hanger to cut off a small piece of the  
grapple;

grapple; there was a great number of lizards which had taken shelter there, and which run out on my introducing the hanger. I then discovered a dark spot, at the distance of more than a foot, within the circumference of the pillar; which, by striking it with the hanger, I found was something stuck fast to the base; after striking it several times, I detached it from its place, and it proved a medal of Vespasian in fine order. AVT. KAIS. ZEBA. OYEEΠ.... The reverse is, *Victoria gradiens; Dextra spicas, sinis. palmam.*

*This medal was shewn to the Royal Society.*

The reversed hieroglyphicks are a proof that this amazing monument was not erected before Pompey's time; and as there is no mention of it in Strabo, or any one of the antient writers that I have met with, it seems plain it was not known before the time of Vespasian. This medal could not by any accident, I think, have been introduced above a foot within the circumference of the shaft; therefore I suppose it was placed there when the pillar was erected, which from thence I conclude to have been done to the honour of that emperor; and perhaps on his restoring the cripple to the use of his limbs.

If you think this paper worth it, you will please to communicate it to the Royal Society, and that of the Antiquaries.

The pillar is exactly shewn, with the pivot it stands upon, with a reference to the spot the medal was found upon, in the view of it that I have sent to England.

I beg you will assure the Society of my respect, and how happy I shall be to execute any of their commands.

And I hope you will rest persuaded of the true consideration, with which

I am,

Dear Sir,

Your most humble servant,

Zante, May 7,  
1767.

Ed. Wortley Montagu.

*XLIII. Part of a Letter from W. Watſon,  
M. D. F. R. S. to John Huxham,  
M. D. F. R. S. at Plymouth, giving  
ſome Account of the late Cold Weather.  
Dated London, 14 February, 1767.*

Dear Sir,

Read Nov. 19, 1767. **T**HIS waits upon you to inform you of the degree of cold we have lately experienced. After as mild a winter as has been known here for many years, the froſt has been intenſe. Until the latter end of December, many of the tender annual exotic plants continued alive; ſuch as the African Marygold, Naſturtium Indicum, and others of this claſs. I ſaw even the plant, uſually called Balm of Gilead, at that time flouriſhing without ſhelter. Mr. Miller of Chelſea, with whom I talked upon this ſubject, informed me, that he had known the like but twice in his life; and that was in the years 1717 and 1722. However, at the beginning of laſt month, January, after ſome ſmart guſts of wind at Eaſt, it began to freeze; and continued increaſing, until the fifth of that month, in the morning, my thermometer, an excellent one made by Mr. Bird, ſtood in the open air ſomewhat under 20'', in the evening it was 29''. It continued thereabouts to the ninth, when in the morning it ſtood at 20'' again, and at night at 21''. On the tenth in the morning it ſtood at 17'', at night at 18''. From this time to the eighteenth it was never below 23'', but frequently



frequently above the freezing point. On the eighteenth at night it stood at  $19''$  and  $\frac{1}{2}$ . On the nineteenth at six in the morning it stood at  $16''$ , at eight in the morning  $15''$   $\frac{1}{2}$ , at eleven at night  $17''$ . On the twentieth at eight in the morning it stood at  $18''$  at eleven at night  $22''$ . On the next day, January twenty-first, the frost broke; the thermometer at four in the afternoon standing at  $36''$ .

In the country it has been observed much colder. On January tenth, at Cardington in Bedfordshire, Mr. Howard, Fellow of the Royal Society, by two thermometers, observed the mercury, at six in the evening, to stand at  $9''$ . And upon the same day the Rev. Mr. Wollaston, at East Dereham in Norfolk, found it so low as  $8''$ . Mr. Ellinet, at Norwich, on the nineteenth of January, found his thermometer, at eight o'clock in the morning, to stand at  $7''$ ; at noon at  $22''$ ; at five in the afternoon  $18''$ ; at ten at night at  $8''$ . Seven degrees therefore is the lowest point, at which I have heard the thermometer has stood any where in England during the late frost.

January tenth was the coldest day at Plymouth, where Dr. Farr, a very ingenious physician, and Mr. Mudge, who has communicated several papers to the Royal Society, each observed his thermometer to stand at  $23''$  and  $\frac{1}{2}$ .

The severe frost of 1739, of which no account appears in the Philosophical Transactions, began December twenty-ninth, when Lord Charles Cavendish's thermometer in his room stood at  $25''$ ; the next evening  $21''$ .

From this time his Lordship placed his instrument out of the window, and at some distance from it; when

in the evening of December thirtieth, it stood at  $17''$ . On the morning of January fifth, it stood at  $13''$ . On the eighth at  $15''$ . The ninth at  $14''$ . Twenty-second at  $19''$ . Twenty-fifth at  $17''$ . On February fifth at  $21''$ . During all this time, the thermometer was seldom above  $32''$ , the freezing point.

Thirteen degrees, therefore, during this frost, was the lowest observation, by Lord Charles Cavendish in Marlborough Street.

I am,

with all possible respect,

Dear Sir,

your most obliged,

and obedient Servant,

W. Watson.

## Abstract from a Meteorological Diary kept at Plymouth.

This contains Observations twice a Day ; the Barometer and Thermometer are made by Siffon, the Rain Machines also, and similar to that described in the Edinburgh Medical Essays.

N. B. One of the Thermometers is kept in a Parlour where there is no Fire, the other without Doors in the Rain Machine, which is never affected by the Sun, but was not examined before or after the Sun set ; so that the lowest State of the Thermometer in these Parts is not ascertained.

J A N U A R Y 1767.

Day	Bar.	Ther. within doors.	Ther. with- out doors.	Wind.	Rain	Weather.
1 9 a.m.	30 20	44		N.	2	Clear
11 p.m.	29 84	45		E. b. N.	3	D° a remark. gulf of wind for some h. in the n.
2 9 a.m.	29 82	42	32	N. E.	2'''	Clear
11 p.m.	29 65	40		N. E.	2'''	D° with frost this night and the last
3 9 a.m.	29 66	40	30	N. N. E.	2'''	Fair ; a few flakes of snow fell thro' the day
11 p.m.	29 77	39		N. E.	2	Clear
4 9 a.m.	29 88	38	30	E.	2'	D° a very sharp air
11 p.m.	29 96	35		E.	2	D°
5 9 a.m.	29 95	33		N. E.	2	D°
11 p.m.	29 91	35		N. W.	2	D°
6 9 a.m.	29 73	35		N. N. W.	2	D°
11 p.m.	29 74	38	33	N. W.	2	Cloudy ; little or no frost
7 9 a.m.	29 64	39		E. b. N.	0	Hazy
11 p.m.	29 56	39		S. W.	1	.230 Rain, heavy ; but towards morn. severe frost
8 9 a.m.	29 61	36		E. b. N.	2	Frost ; clear with a very sharp air
11 p.m.	29 70	35		N. E.	2	Heavy falls of snow
9 9 a.m.	29 70	35		N.	1	Slight falls of snow at times
11 p.m.	29 72	35		N. W.	2	Clear and sharp air
10 9 a.m.	29 60	34	23 5	N. W.	2	D°
11 p.m.	29 46	34		S. W.	2'''	Cloudy ; and towards morning rain

Jan. Day	Bar.	Ther. within doors.	Ther. with- out doors.	Wind	Rain	Weather
11 9 a. m.	29 22	33	24	N.	1	.110 Hail; sleet; and after, most heavy fall of snow
11 11 p. m.	29 20	33		N.	1	Cloudy; snow in 3 h. cov. upw. 6 inch. deep
12 9 a. m.	29 17	32	24	N. W.	1	Clear wind with the sun; a very fine day
11 11 p. m.	29 15	35		S. W.	2	Cloudy. N. B. Thermometer at noon 24
13 9 a. m.	28 93	37		S. S. E.	2'''	Cloudy; snow every where melt. by the thaw
11 11 p. m.	28 67	39		S. E.	3	Rain heavy through the night; stormy
14 9 a. m.	28 67	40		S.	2	.960 Heavy rain in the morn. showery thro' the day
11 11 p. m.	28 90	42		S.	2	Showery
15 9 a. m.	29 30	44		S. S. E.	2''	Cloudy, with sun-shine at times
11 11 p. m.	29 36	45		S. S. E.	2'''	Cloudy
16 9 a. m.	29 42	44		S. S. E.	2'''	.320 Showery
11 11 p. m.	29 55	44		E.	2	Cloudy
17 9 a. m.	29 70	40		N. E.	2	Cloudy; with a sharp cold air
11 11 p. m.	29 80	44		E. b. N.	2	D°
18 9 a. m.	29 82	36		N. E.	2	Fair; air very sharp
11 11 p. m.	29 90	35		N. E.	2	Clear and frost
19 9 a. m.	29 90	35		N. N. W.	1	.070 Clear till 4 p. m. then showery
11 11 p. m.	29 92	38		N.	0	Clear and frost
20 9 a. m.	30 0	36	27	N.	0	D°
11 11 p. m.	30 8	38	34	W.	1	Cloudy; thaw; towards morning rain

Depth of Rain which has fallen at Plymouth during  
the Year 1766.

1766	Dec. Inc. Parts	Barom. hig. state	Barom. low. state
January	0 .165	30 74	29 83
February	2 .590	30 65	29 40
March	1 .690	30 45	28 83
April	3 .780	30 43	29 26
May	2 .600	30 23	29 21
June	3 .880	30 21	29 33
July	3 .550	30 3	29 56
August	1 .600	30 10	29 44
September	2 .630	30 20	29 45
October	2 .650	30 45	29 08
November	4 .660	30 50	28 62
December	3 .280	30 44	29 14
Inches	35 .075		

N. B. In the latter end of December 1766, the weather was so mild, and had been so in the preceding weeks, that ripe strawberries were gathered in my garden at the Royal Hospital.

Meteorological Register, kept at the Royal Hospital near Plymouth, continued from January 20, to March 1, 1767.

Day Jan.	Bar.	Th <sup>r</sup> .	Wind	Rain	Weather	Remarks
21	9 a. m. 29	97 39	W. b. S.	2	Slight showers	
	11 p. m. 29	92 40	W. b. S.	1	Cloudy	
22	9 a. m. 29	67 42	W. b. S.	2''	Small rain constant thro' the day	
	11 p. m. 29	74 44	N. W.	2	Cloudy	
23	9 a. m. 29	94 44	N.	2	Showery, but clear for the most part	
	11 p. m. 29	0 45	N.	2	Cloudy weather	Highest state of Barom. } 30 5
24	9 a. m. 29	90 46	S. W.	2	Small rain through the day	Lowest state of Barom. } 29 4
	11 p. m. 29	81 48	S. W.	2'	D°	
25	9 a. m. 29	72 49	S. W.	2'''	Cloudy, with small rain at times	
	11 p. m. 29	65 49	S. W.	2'''	D°	Highest state of Therm. } 53
26	9 a. m. 29	62 49	S. W.	2'''	Showers in the forenoon, afterw. clear	Lowest state of Therm. } 39
	11 p. m. 29	68 49	W. b. S.	2	Cloudy	
27	9 a. m. 29	50 50	S.	3	Rain heavy and constant thro' the day	
	11 p. m. 29	58 50	S. S. E.	2''	D°	
28	9 a. m. 29	67 50	N. N. E.	2	Small rain through the day	The quantity of rain is very considerable during this period ; and yet, on looking back into the re- gister, I find that in the month of March 1765, .
	11 p. m. 29	50 49.5	N. E.	1	Rain	Inc.
29	9 a. m. 29	77 46	S. S. E.	2	Rain from noon till 4 p. m.	the total is 10.820 and taking in from the 23d of February, to the 2d of April 1765, .
	11 p. m. 29	77 48	S. S. E.	2	Cloudy	Inc.
30	9 a. m. 29	73 50	S. W.	2	Fair till 11 a. m.	
	11 p. m. 29	72 51	S. W.	2	Rain constant since 11 a. m.	
31	9 a. m. 29	73 52	S. S. W.	2	Rain constant, with hazy weather	
	11 p. m. 29	78 53	S.	2	Small rain	
Feb.						
1	9 a. m. 29	90 52	S. S. E.	1	Fog, with small r. aft. heavy thro' day	1765, viz. 39 days, Inc.
	11 p. m. 29	74 53	S. S. W.	2	Cloudy	the amount } 13.900
2	9 a. m. 29	09 52	S. E.	1	Hazy weather, with rain at times	is }
	11 p. m. 29	91 53	S. E.	1	D°	
3	9 a. m. 29	83 51	S. E.	2	Fair, with sun-shine thro' the day	A quantity equal, I suppose, to what usually falls in 6 months, in most parts of England, being nearly fourteen inches.
	11 p. m. 29	72 52	E.	1	Clear	
4	9 a. m. 29	60 51	E.	2	Fair through the day	
	11 p. m. 29	56 49	E. b. S.	2	Clear	
5	9 a. m. 29	54 51	E.	2	D°	
	11 p. m. 29	59 50	E.	2'	Cloudy	
6	9 a. m. 29	60 48	E. b. S.	2	Small rain, with hazy weather	
	11 p. m. 29	41 51	E.	2''	Rain	
7	9 a. m. 29	23 51	E.	2	Hazy, with showers	
	11 p. m. 29	15 51	S.	2	Heavy showers, with squalls.	
VOL. LVII.					M m m	Day

Day	Feb.	Bar.	Th.	Wind	Rain	Weather
8	9 a. m.	29 15	51	S. W.	2"	.600 Hazy showers through the day
	11 p. m.	29 13	50.5	S. W.	2	Cloudy
9	9 a. m.	29 32	51	E.	1	Hazy weather, with rain till 1 p. m.
	11 p. m.	29 37	51	S. W.	2	Cloudy, but afternoon fair
10	9 a. m.	29 17	51	S. W.	2"	Thunder showers heavy, with squalls
	11 p. m.	29 48	51	W. b. W.	2	Showery
11	9 a. m.	29 23	51	S. E.	2	.770 Rain const. from 11 a. m. thro' the d.
	11 p. m.	29 20	51	S. S. W.	3	Rain, with very heavy squalls
12	9 a. m.	29 35	50	N.	2	Slight showers, with sun-shine between
	11 p. m.	29 52	49	N.	1	Clear and serene
13	9 a. m.	29 22	49	S. E.	2"	Rain constant; hazy, w. var. to S. W.
	11 p. m.	29 17	50	W.	2"	.990 Heavy showers
14	9 a. m.	29 34	50	W.	2"	Hail storms, with heavy showers
	11 p. m.	29 30	51	S. W.	2	Cloudy
15	9 a. m.	29 54	51	S.	2	.670 Rain very heavy till the afternoon
	11 p. m.	29 57	51	W. b. S.	2	Clear
16	9 a. m.	29 53	50	S. S. W.	2	Cloudy, with small rain at times
	11 p. m.	29 63	50	S. W.	2	Cloudy weather
17	9 a. m.	29 28	51	S.	2	.450 Rain constant till 2 p. m.
	11 p. m.	29 32	50	S. W.	2	Showery
18	9 a. m.	29 38	51	S. W.	2	Rain till noon
	11 p. m.	29 67	51	S. W.	2	.420 Showery; but clear for the most part
19	9 a. m.	29 67	51	S. W.	2"	Clear
	11 p. m.	29 68	51	S. S. W.	3	Rain in the night very heavy
20	9 a. m.	29 26	51	S. E.	2"	.710 Rain constant through the day
	11 p. m.	29 4	51	S. W.	3	Hail-ft. with most heavy squalls of w.
21	9 a. m.	29 32	51	S. E.	2	.300 Fair, with sun-shine
	11 p. m.	29 14	49	S.	2	Rain since 5 p. m.
22	9 a. m.	29 43	48	W. b. N.	2	Fair
	11 p. m.	29 65	48	W.	2	Showers in the even. at present clear
23	9 a. m.	29 80	48	S.	1	Clear
	11 p. m.	29 87	49	W. b. N.	2	.716 Cloudy; in the night, heavy rain
24	9 a. m.	29 86	50	W.	2	Rain drizzling through the forenoon
	11 p. m.	29 36	50	W. b. N.	2	Cloudy
25	9 a. m.	29 74	51	S. W.	2"	.510 Rain constant till 3 p. m.
	11 p. m.	29 70	51	W.	2	Showery
26	9 a. m.	29 51	51	S. W.	2"	Rain drizzling constant through the day
	11 p. m.	29 44	51	S. W.	3	Rain; wind at S. W. a storm
27	9 a. m.	29 5	52	N. W.	2"	Black and cloudy weather; but little r.
	11 p. m.	29 56	51	N. W.	2"	D°
28	9 a. m.	29 6	49	N. N. W.	2"	.284 Showery in the morn. afterw. Hail-ft.
	11 p. m.	29 90	49	N. N. W.	2"	Show. at times; but fair for the most part
Total					94	.940 or near 10 inches of rain in 39 days

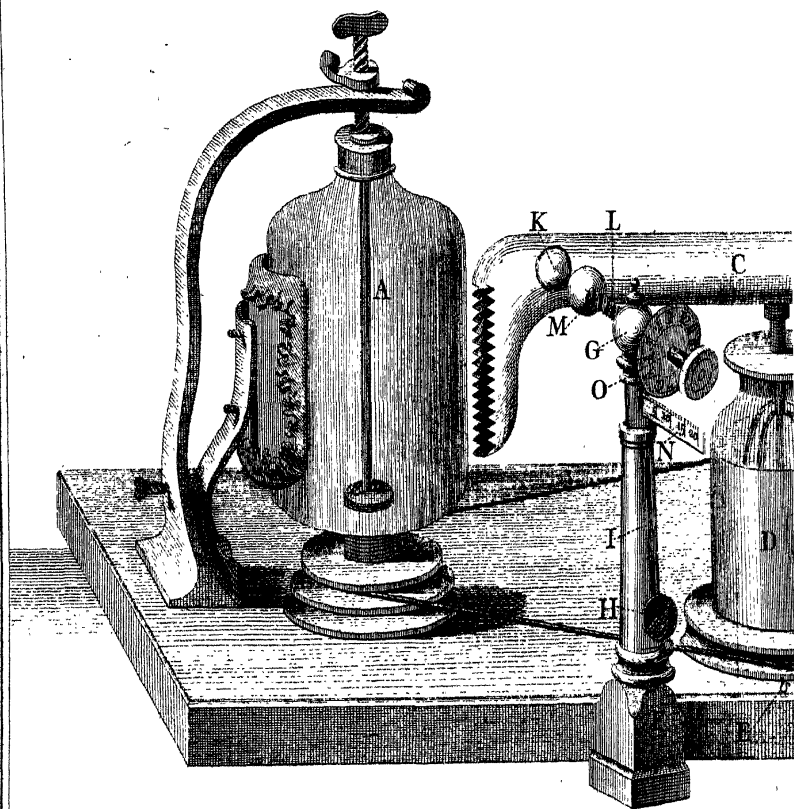
## Remarks

N. B. The machine is placed free of eddy winds, at a very considerable distance from any building, except to the North, where, however, it is sixty feet distant, and from that quarter we have little or no rain.

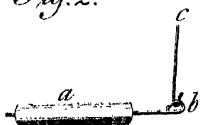


*Fig. 1.*

c  
b



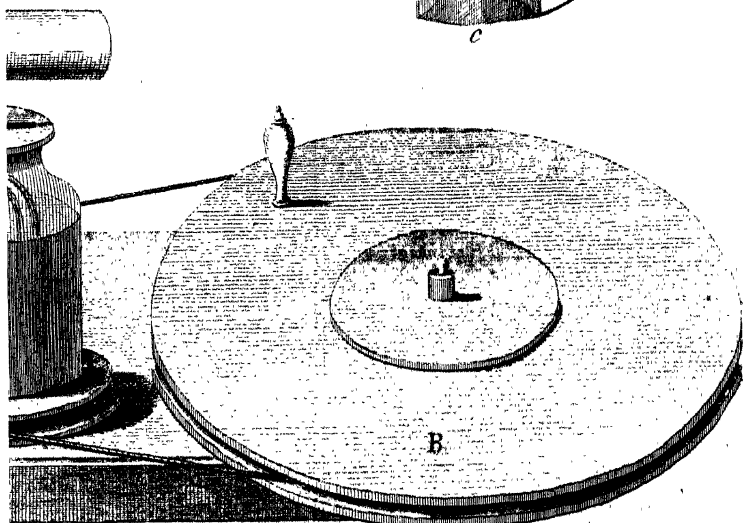
*Fig. 2.*



*Fig. 3.*



*Fig. 4.*





Received August, 1767.

XLIV. *Description of an Electrometer invented by Mr. Lane; with an Account of some Experiments made by him with it: In a Letter to Benjamin Franklin, LL. D. F. R. S.*

Aldersgate-Street, October 15, 1766.

S I R,

Read Nov. 26, 1767. **B**EING employed in some electrical enquiries about the beginning of the year 1762, it occurred to me, that many experiments on this subject might be made with a much greater degree of precision, if we could determine, with any tolerable accuracy, the comparative quantity of electric fluid, with which, for any given experiment, the coated phial is impregnated.

An instrument, which I have contrived for this purpose, may not improperly be called an Electrometer. I have herewith sent you a drawing thereof [TAB. XX.] with the machine \* to which I have fixed it.

\* This portable machine is the contrivance of Mr. Read, mathematical instrument maker at Knightbridge, near London.

FIGURE I.

- A. The cylindrical glass of the machine, used instead of a globe. The cylindrical part of the glass is six inches in length, and sixteen in circumference.
- B. The wheel, at every turn of which the cylindrical glass revolves four times.
- C. The conductor.
- D. The coated phial.
- E. A brass wire loop, passing through the wood work to a tin plate, on which the coated phial stands.
- F. The pillar of the Electrometer made of wood, bored cylindrically about  $\frac{2}{3}$  of its length, and rendered electrical, by being long baked in an oven, and then boiled in linseed oil, and again baked. At first the pillar was made of brass, which, though it served very well to determine the electric stroke for medical purposes, yet was defective in many experiments, as the table thereby became a ready conductor.
- G. Brass work, having its lower part inclosed within the bore of the pillar.
- H. A screw, which passes through the brass work near the bottom, and fixes it in the pillar.
- I. A groove for the screw H to move in, when the Electrometer is moved higher or lower, as the different heights of different condensing phials may require.
- K. A well polished hemispherical piece of brass, fixed to the conductor.

L. A

- L. A steel screw, passing through the top of the brass work, whose threads are distant nearly  $\frac{1}{4}$  of an inch from each other.
- M. A well polished spherical piece of brass, fixed to the screw L, and opposite to K. The polish of K and M will often be destroyed by large electrical explosions, and it should again be restored, particularly where the experiments require accuracy.
- N. A scale, with divisions equal to each turn of the screw.
- O. A circular plate fixed to, and moving with the screw, pointing at each turn to the division upon the scale. This plate is also divided into twelve, to denote the parts of each turn.

The principle on which the Electrometer acts is very simple, being merely this; the coated phial is hereby rendered incapable of accumulating and retaining any more than a certain quantity of the electric fluid, for any intended experiment, when a metallic or non-electric communication is made from the screw H to the wire loop E of the machine, and that quantity will be proportionate to the distance of K and M from each other, and consequently the explosion and stroke will thereby be regulated.

Thus if a person holds a wire fastened to the screw H in one hand, and another wire fixed to the loop E in the other, he will perceive no stroke, if K and M are in contact, notwithstanding the cylindrical glass A acts strongly. But if, by turning the screw L, the ball M is distant from K  $\frac{1}{100}$  part of

an inch, a very small stroke will be perceived, with an explosion from K. to M; and if K and M are distant one inch from each other, the quantity of the electric fluid, at the time of the explosion, will be increased 100 times: for example, it appears by experiment, that, if the explosion happens after 4 turns of the wheel B, when M is distant from K  $\frac{1}{2}$  of an inch, or 1 turn of the screw; the same will happen at 8 turns of the wheel, when M and K are distant 2 turns of the screw, or  $\frac{1}{2}$  of an inch; and if K and M are distant 3 turns of the screw, the turns of the wheel will be 12 at the time of the explosion; the same proportion will continue so far as the distance of K. and M is equal to the condensing power of the coated phial without wasting. By wasting, I mean when the phial is so fully charged, that part of the electric fluid escapes from the mouth of the bottle, or from the conductor into the air, or to some adjacent non-electric. The number of turns of the wheel, when K and M are at any of the above distances, will be more or less in proportion to the state of the air, the cylindrical glass, the cushion against which the glass is rubbed, or the coated phial; which last will not give so great an explosion when the air is damp as when dry.

The fewer the number of turns of the wheel, at any given distance, the better the machine worketh. Thus a comparative difference between any two machines may be determined.

A wire in general is better than a chain, unless the chain is held very tight; particularly in very small strokes, the electric fluid will be lost in passing from link to link of the chain.

By

By experiment it also appears, that the quantity of electric fluid, at every explosion, will be proportionate to the quantity of coated glass, either as to the size of the coated phial, or to the number of phials added. For example, if the phial D has half of the coating on each side of the glass taken off, the explosion will happen after half the number of turns of the wheel, at any of the above distances; and if a phial, with twice the quantity of coated glass, is employed instead of D, the number of turns of the wheel will be double; the same will happen if two coated phials, each equal to D, are used; and if three phials, the number of turns will be triple, &c.

The phial D, used in the following experiments, contains about 80 square inches of coating on the inside, and also on the outside of the glass; the mouth being stopped with wood, prepared like the pillar, and the coating not too near the mouth of the phial, to prevent the electric fluid's wasting, and thereby the phial may be more fully charged.

As K is part of the conductor, and of M the electrometer, the distance between them is the distance of the electrometer from the conductor; whence it will be readily understood, when I relate the distance of the electrometer, in any experiments. For example, the electrometer at 20, that is, M, is 20 turns of the screw distant from K, or  $\frac{2}{3}$  of an inch.

That lightning and electricity are of very near affinity, if not the same, evidently appears from the many discoveries you have made; and as the following experiments tend to confirm the same, as well as



to illustrate the use of the electrometer, I hope they will not be unacceptable.

### EXPERIMENT I.

A piece of moist tobacco-pipe clay, rolled cylindrically, *a*. fig. 2. about an inch in length, and about 2 or  $\frac{3}{8}$  of an inch in diameter, having a piece of wire thrust into each end, *b b*, distant about  $\frac{1}{2}$  of an inch from each other, with the solid clay between, and the end of one of the wires, *c c*, fixed to the loop of the machine E, and the other fixed to the small screw of the electrometer H, will, with an explosion at 20 of the electrometer, be inflated as in fig. 3. or if the clay is too dry, or the quantity of electricity too great, it will burst in pieces, leaving only the clay concave near the ends of the wires; and though the experiment will in appearance differ, yet it will always leave evident signs of an explosive power, or sudden rarefaction, excepting when the wires in the clay are at too great a distance from each other; then the electric fluid will only run over its moist surface. If, instead of clay, a mucilaginous vegetable paste is used, as wheat-flower and water, &c. the experiment will appear the same.

### EXPERIMENT II.

Take a piece of common tobacco-pipe hard-baked, as used for smoking, about an inch in length; fill the bore with clay, and put wires into each end, as in fig. 2. which applied in the same manner to the machine, will burst into many pieces,  
at

at 20 of the electrometer ; sometimes the pieces will be driven near ten feet from the machine.

### EXPERIMENT III.

A small square piece of Portland stone, with holes drilled at each end so as to admit the wires, was in like manner burst in pieces, when a second coated phial was added to increase the stroke.

The iron cramps in stone buildings are similar to the wires, and when a building is struck by lightning produces a similar effect. I observed, that when the tobacco pipe, or stone, was damp, the experiment succeeded better than when dry ; and I frequently found, that either of them, after being first dipped in water, would be broken with a less explosion than before.

This observation is different from the received opinion of many, not well acquainted with electricity, that lightning is less likely to do mischief after a shower of rain than before : so far may be true, that the rain will bring down some of the lightning, and also render thatched houses, &c. less likely to take fire, but will not assist buildings that have metallic ornaments near their tops, as the weather-cocks of churches, &c.

As a metallic conductor from the tops of buildings to the earth will prevent the effects of lightning on them, so will the smallest wire prevent the effects of electricity on the stone, or tobacco-pipe, when in contact with the two wires, *c c*, fig. 2.

If the tobacco-pipe, instead of clay, is filled as above, with an electric substance, as wax, powdered glass, or with any non-electric substance, inferior to metals as a conductor, it will be burst in pieces with nearly the same quantity of the electric fluid.

As the above experiments succeeded better when the stone or clay were previously dipped in water than before, I was induced to try water only.

#### EXPERIMENT IV.

Having made a hole, without any cracks on the side, through the bottom of the phial, *a*, fig. 4. which may easily be done if the phial is conical at the bottom, as in the figure, by holding the phial inverted in one hand, and with the other striking a pointed steel wire against the apex of the cone.

Through this hole I passed a wire, *b*, and filled the bottom, *c*, with melted sealing wax, leaving the other end of the wire out, at *d*; when the wax was cold, the phial was about  $\frac{2}{3}$  filled with water, and stopped with a cork, through which a wire, *e*, was passed downwards, till the points of the two wires were distant from each other about  $\frac{1}{10}$  of an inch, as near as my eye could determine a wire from the electrometer was fixed to *e*, and another from the loop of the machine was fixed at *d*; by an explosion, at 20 of the electrometer, the phial burst in

in pieces, the top falling from the bottom near the point of the lower wire. Another phial was fitted in the same manner, and the cork cut longitudinally, that the air might freely pass at the time of the explosion, but this made no sensible difference: often times the phial is so cracked as to resemble radii from a center.

If oil is used instead of water, the event will be the same.

The quantity of electricity necessary to burst the phial, appears to vary more in proportion to its thickness than its size; many phials of various sizes may be broken at 10 of the electrometer, while others, nearly of the same size, remain sound, with a stroke at 30, or even more.

I generally found green glass more difficult to break than white.

When the phial is not broken by the electric stroke, the agitation of the water may be sensibly observed at the instant of the explosion, and the electric spark evidently seen to pass through the water, from the point of one wire to the other.

This remarkable appearance of the electric fluid's passing through water may be observed, when the electrometer is at a smaller distance from the conductor, if the wires are nearer to each other.

I have broken many phials by the electric strokes as above-mentioned, when the wires have been at the various distances, of above 1 inch to  $\frac{1}{20}$  of an inch from each other, as near as my eye could determine; but the distance of about  $\frac{1}{20}$  of an inch I usually prefer.

The above experiments I have often repeated, and may therefore be relied on : want of leisure has prevented me from pursuing them more minutely. But I hope they will serve as hints to others of more abilities and leisure, than

Your respectful

humble servant,

T. Lane.

Received

Received in October, 1767.

*XLV. Of the Increase and Mortality of  
the Inhabitants of the Island of Madeira.  
By Dr. Thomas Heberden, F. R. S.*

Read Nov. 26, 1767. **W**HEN I consider the number of people in the Madeira, and the state of the inhabitants, I know no place more proper for forming an estimate of the increase and mortality of mankind, than this island; for the number of persons is upwards of 60,000, all of whom may be supposed to live and die in the same place where they received their existence; the accession of strangers and the egression of the natives being so equally inconsiderable, that if the one doth not exactly counterbalance the other, the difference may justly be neglected, as of no consequence in the general calculation.

This has excited my curiosity; and, by my interest with the vicar-general of this diocese, I have procured a survey from house to house in each of the respective parishes; from which, and the parish registers, I have deduced the adjoined account.

An Hypothesis.

The number of persons in this island, in the year 1743, was 48234 of seven years old and upwards.

Now supposing the minors were in the same proportion then, as in this present year, the total of the inhabitants was 53,057. Therefore, by the rule of anatocism, they have increased at the rate of 1.0082 *per cent. per annum*; and by the same rule do double in 84 years 4 months and 25 days.

From an exact survey, made in the beginning of the year 1767, the number of inhabitants on the island of Madeira, was as follows;

Persons of seven years old and upward	58669
Persons under seven years of age	5945
Total	<u>64614</u>

Anno	Christened		Buried		Wedd.
	Males	Fem.	Males	Fem.	
1759	1021	905	542	594	438
1760	1198	1111	643	713	421
1761	1035	1022	837	909	513
1762	1128	1125	662	704	491
1763	1118	1115	540	578	476
1764	1112	1085	620	705	469
1765	1183	1143	618	649	495
1766	1172	1138	506	531	462
	<u>8967</u>	<u>8644</u>	<u>4968</u>	<u>5383</u>	<u>3765</u>

Total      17611      10351

Christened

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Christened in 8 years	17611	Medium for each year	2201 $\frac{3}{8}$
Buried in 8 years	10351	Medium for each year	1293 $\frac{7}{8}$
Octennial increase	<u>7260</u>	Annual increase	<u>907 <math>\frac{1}{4}</math></u>

Proportion of the yearly births to the number of persons,	as	1	to	29,35
of the yearly burials to the number of persons,	as	1	to	49,89
of births to burials		100	to	58,77
of males born, to females		100	to	96,39
of females buried, to males		108,33	to	100

Weddings each year, at a medium		470 $\frac{5}{8}$
Proportion of Weddings to births,	as 1	to 4,68
of Weddings to burials,	as 1	to 2,75

### Mortality of the Seasons.

Winter		Spring		Summer		Autumn	
January	93	April	108	July	129	October	87
February	84	May	105	August	135	November	111
March	132	June	120	September	84	December	84
	<u>309</u>		<u>333</u>		<u>348</u>		<u>282</u>

The mortality of spring and summer, to that of autumn and winter, as 115 to 100.

N. B. This calculation of the mortality of the seasons is not deduced from the whole number of Inhabitants on the island, as I could not procure authentic materials to proceed with exactness; the number of the persons, from which it is calculated, is 6880.

Received



Received November 19, 1767.

**XLVI.** *An Account of some very large Fossil Teeth, found in North America, and described by Peter Collinson, F. R. S.*

Read Nov. 26, 1767. **I** Perswade myself it will not be unacceptable to this Learned Society, to receive the best intelligences I can collect of the teeth, and bones of elephants, found in North America, in the year 1766, which are now offered for your inspection.

George Croghan, Esquire, who is a deputy of Sir William Johnson, the King's superintendant of Indian affairs in America, in the course of his navigation down the great river Ohio, after passing the Miame river, in the evening came near the place where the elephants bones are found, about four miles south-east of the Ohio, and about six hundred miles distant from and below Pittsburgh, from the nearest sea-coast at least seven hundred miles. Next morning he met with a large road, which the buffaloes had beaten, wide enough for two waggons to go a-breast, leading strait into the great licking-place, to which the buffaloes and all the species of deer resort, at a certain season of the year, to lick the earth and water from salt springs, that are impregnated with nitreous particles; whether to cleanse their

their stomachs, or for what other purpose, is submitted to the sentiments of the Society.

Esquire Croghan had been here some years before, and gave some account of the monstrous bones, and teeth, found at this place, called by the Indians The Great Buffaloes Lick ; but being now more at leisure, he carefully examined all its surrounds, and discovered under a great bank, on the skirts of the Lick, five or six feet below the surface, open to view, a prodigious number of bones and teeth, specimens of which now lie before the Society, belonging to some of the largest-sized animals ; by the quantity, he computes there could not be less than thirty of their skeletons.

By their great teeth, or tusks, of fine ivory, some near seven feet long ; every one that views them, I believe, will not hesitate to conclude they belong to elephants.

It is very remarkable, and worthy observation, none of the molares, or grinding teeth of elephants, are discovered with these tusks ; but great numbers of very large pronged teeth of some vast animals are only found with them, which have no resemblance to the molares, or grinding teeth, of any great animal yet known.

As no living elephants have ever been seen or heard of in all America, since the Europeans have known that country, nor any creature like them ; and there being no probability of their having been brought from Africa, or Asia ; and as it is impossible that elephants could inhabit the country where these bones and teeth are now found, by reason of the severity of

the winters, it seems incomprehensible how they came there.

I conclude, many of this learned Society are not unacquainted with the fossil elephants teeth annually found in Siberia, lodged in the banks of the great river Oby, and other rivers of that country.

On the system of the deluge, it has been conjectured, that, as the extensive kingdom of Siberia lies behind the native country of the elephants in Asia, from West to East, and to the North, by the violent action of the winds and waves, at the time of the deluge, these great floating bodies, the carcasses of drowned elephants, were driven to the Northward, and, at the subsiding of the waters, deposited where they are now found. But what system, or hypothesis, can, with any degree of probability, account for these remains of elephants being found in America, where those creatures are not known ever to have existed, is submitted to this learned Society.

Nov. 4,  
1767.

P. S. The Bishop of Carlisle presented to the Royal Society, on the 27th of February, 1766, some fossil teeth and bones from Peru, which have some analogy with the before-mentioned, not so recent, but much more petrified; the pronged teeth are like to agate.

*A List of the Teeth and Bones sent over by George Croghan, Esquire, February 7, 1767, from Philadelphia.*

To Lord Shelburne.

Two of the largest tusks, or teeth, one whole and entire, above six feet long, the thickness of common elephants teeth of that length.

Several very large forked or pronged teeth; a jaw-bone, with two of them in it.

To Doctor Franklin.

Four great tusks, of different sizes.

One broken in halves, near six feet long.

One much decayed, the center looks like chalk, or lime.

A part was cut off from one of these teeth, that has all the appearance of fine white ivory.

A joint of the vertebræ.

Three of the large pronged teeth; one has four rows of fangs.

Besides the above, Captain Owry, an Officer who served in the country during the last war, now living at Hammermith, hath a small tusk, as if of a calf elephant, the surface of a fine shining chestnut colour, and a recent look; and a great pronged tooth, larger than any of the above, which were also brought from the same licking place.

XLVII. *Sequel to the foregoing Account of the large Fossil Teeth.* By P. Collinson, F. R. S.

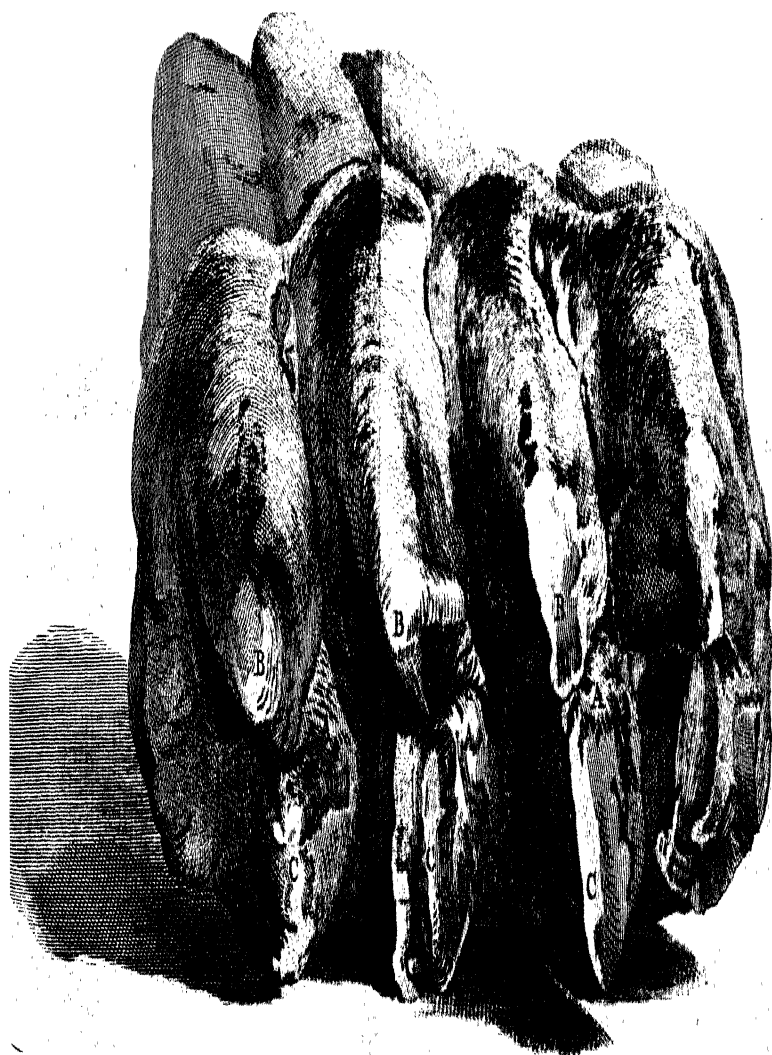
Read Dec. 10, 1767. **I**N my observations on the long teeth and grinders, at the last meeting of this Society, I forebore giving my sentiments on these remains of great animals found at the Great Lick, near the river Ohio, being willing the Society should determine for themselves.

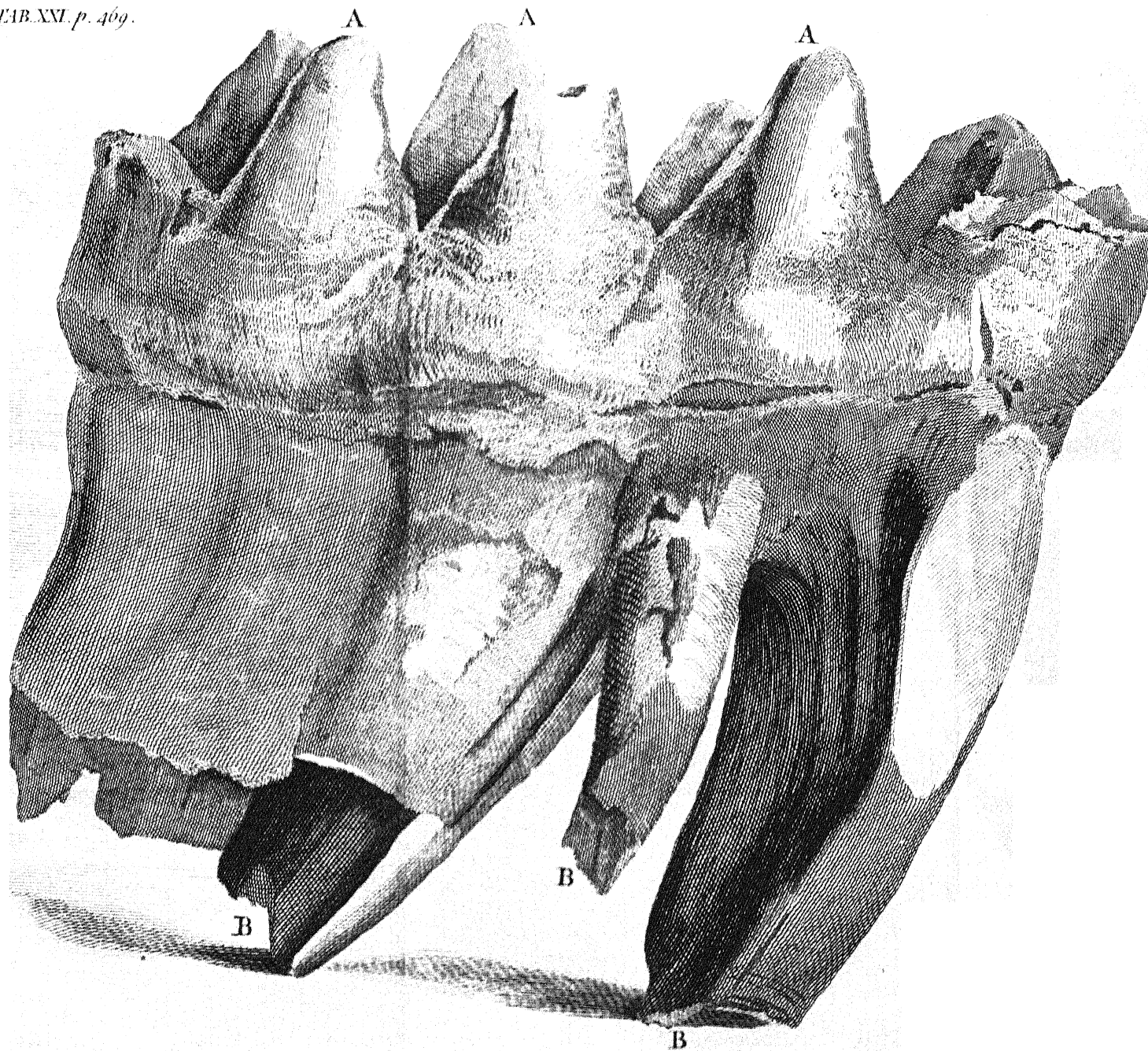
As I perceived one of the long teeth, or tusks, was channelled or ribbed, near the larger end, I was in some doubt, if peculiar to the elephant. To satisfy myself, I went to a warehouse, where there were teeth of all sorts and sizes for sale; on examining them, I found as many ribbed or channelled, as plain and smooth, so that now, I have no difficulty to pronounce them, agreeing in all respects, with the elephants teeth from Africa and Asia.

But as the biting or grinding teeth, found with the others, have no affinity with the molares of the elephant, I must conclude, that they, with the long teeth, belong to another species of elephant, not yet known; or else that they are the remains of some vast animal, that hath the long teeth, or tusks, of the elephant, with large grinders peculiar to that species, being different in size and shape from any other animal yet known. I had one of these grinders, that weighed near four pounds, with as fine an enamel on it, as if just taken out of the head of the creature.

The











The elephant is wholly supported by vegetables; and the animal to which these grinding teeth belong, by their make and form, seemed designed for the biting and breaking off the branches of trees and shrubs for its sustenance; and if I may be allowed to conclude from analogy, that the great heavy unweildy animals, such as elephants, and the rhinoceros, &c. are not carnivorous, being unable, from want of agility and swiftness, to pursue their prey, so are wholly confined to vegetable food; and for the same reason, this great creature, to which these teeth belong, wherever it exists, is probably supported by browsing on trees and shrubs, and other vegetable food.

#### Explanation of the Fig. in Tab. XXI. and XXII.

Tab. XXI. is a side view of a large pronged tooth, that weighed three pounds and three quarters, and was eighteen inches round, and four inches thick. It is now in the possession of the Right Honourable Earl Bute. A. A. A. The prominences of the top or crown. B. B. B. The prongs by which it has been fastened in the sockets.

Tab. XXII. represents the top of the tooth; A. A. A. are the sinus's, or concavities between B. B. B. and C. C. C. or the two rows of prominences that form the crown of the tooth.

XLIX. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1766, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. nuper Præses: By William Hudson, Societatis Regiæ & clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Præfectus et Prælector Botanic.*

Read Dec. 10, 2201  
1767.

- A** Grostis *spica venti*, petalo exteriore exferente aristam rectam strictam longissimam. Lin. Sp. pl. ed. II. 91. Hudf. Fl. Angl. 26.  
Gramen segetum altissimum, panicula sparsa. Bauh. pin. 3.
- 2202 Agrostis *mil ea*, petalo exteriore arista terminali recta stricta mediocri. Lin. Sp. pl. ed. II. 91.
- 2203 Aira *cæspitosa*, foliis planis, panicula patente, petalis basi villosis aristatique, arista recta brevi. Lin. Sp. pl. ed. II. 96. Hudf. Fl. Angl. 29.  
Gramen segetum, panicula arundinacea Bauh. pin. 3. Theatr. 35.
- 2204 Aira

- 2204 *Aira caryophyllea*, foliis setaceis, panicula divaricata, floribus aristatis distantibus. Lin. Sp. pl. ed. II. 97. Hudf. Fl. Angl. 31.  
Gramen paniculatum, locustis purpureo-argenteis, annuum. R. Syn. 407.
- 2205 *Alopecurus pratensis*, panicula cylindrica spiciformi, glumis villosis, culmo erecto. Hudf. Fl. Angl. 23.  
*Alopecurus* culmo spicato erecto, glumis villosis. Lin. Sp. pl. ed. II. 88.  
Gramen phalaroides spica molli. Bauh. pin. 4.
- 2206 *Alopecurus agrestis*, spica cylindrica longissima, glumis glabris, culmo erecto. Hudf. Fl. Angl. 23.  
*Alopecurus* culmo spicato erecto, glumis nudis. Lin. Sp. pl. ed. II. 89.  
Gramen typhoides, spica angustiore. Bauh. pin. 4.
- 2207 *Alopecurus geniculatus*, culmo spicato infracto. Lin. Sp. pl. ed. II. 89. Hudf. Fl. Angl. 24.  
Gramen aquaticum geniculatum spicatum. Bauh. pin. 3.
- 2208 *Alopecurus bulbosus*, spica cylindrica, culmo erecto, radice bulbosa. Hudf. Fl. Angl. 24.  
Gramen myosuroides nodosum. R. Syn. 397.
- 2209 *Alopecurus Monspeliensis*, panicula subspicata, glumis scabris, corollis aristatis. Lin. Sp. pl. ed. II. 89.  
Gramen alopecuroides Anglo-britannicum maximum. Bauh. pin. 4.
- 2210 *Alopecurus paniceus*, panicula subspicata, glumis villosis, corollis aristatis. Lin. Sp. pl. ed. II. 90.

- Gramen alopecurum minus, spica virecente  
divulsa. Barr. ic. 115. f. 1.
- 2211 Anthoxanthum *odoratum*, spica ovato-oblonga,  
flosculis subpedunculatis arista longioribus.  
Lin. Sp. pl. ed. II. 40. Hudf. Fl. Angl.  
10.
- Gramen pratense spica flavescente. Bauh.  
pin. 3.
- 2212 Avena *fativa*, paniculata, calycibus dispermis,  
feminibus lævibus. Lin. Sp. pl. ed. II.  
118.
- Avena alba. Bauh. pin. 23.
- 2213 Avena *nuda*, paniculata, calycibus trifloris, re-  
ceptaculo calycem excedente, petalis dorso  
aristatis. Lin. Sp. pl. 118. Hudf. Fl.  
Angl. 41.
- Avena *nuda*. Bauh. pin. 23. R. Syn. 289.
- 2214 Avena *fatua*, paniculata, calycibus trifloris,  
flosculis omnibus basi pilosis, aristis totis  
lævibus. Lin. Sp. pl. ed. II. 118. Hudf.  
Fl. Angl. 41.
- Avena sylvestris pilosa, aristis recurvis. Hist.  
Ox. III. 209. t. 7. f. 5.
- 2215 Avena *fragilis*, spicata, flosculis subquaternis  
calyce longioribus. Lin. Sp. pl. ed. II.  
119.
- Gramen lollaceum spurium hirsutum, aristis  
geniculatis. Barr. ic. 905. f. 1, 2, 3.
- 2216 Briza *eragrostis*, spiculis lanceolatis, flosculis  
viginti. Lin. Sp. pl. ed. II. 103.
- Gramen paniculis elegantissimis. Bauh. pin.  
2. Hist. Ox. III. 204. t. 6. f. 52.
- 2217 Briza *pinnata*, spica disticha, spiculis ovato-  
lanceolatis.

- Gramen filiceum, paniculis integris. Bocc.  
Sicil. 62. t. 33. f. 2.
- 2218 *Bromus secalinus*, panicula patente, spiculis  
ovatis, aristis rectis. Lin. Sp. pl. ed. II.  
Hudf. Fl. Angl. 39.
- Gramen avenaceum segetate majus, gluma  
turgidiore. Hist. Ox. III. t. 7. f. 17.
- 2219 *Bromus mollis*, panicula erectiuscula, spicis  
ovatis, aristis rectis, foliis mollissime villosis.  
Lin. Sp. pl. ed. II. 112.
- Gramen avenaceum pratense, panicula squa-  
mata et villosa. Hist. Ox. III. 213. t. 7.  
f. 18.
- 2220 *Bromus rubens*, panicula fasciculata, spiculis  
subsessilibus villosis, aristis erectis. Lin.  
Sp. pl. ed. II. 114.
- Gramen panicula molli rubente. Bauh. hist. II.  
266.
- 2221 *Bromus giganteus*, panicula nutante, spiculis  
quadrifloris aristis brevioribus. Lin. Sp. pl.  
ed. II. 114. Hudf. Fl. Angl. 40.
- Gramen sylvaticum glabrum panicula recurva.  
Vaill. parif. 93. t. 18. f. 3.
- 2222 *Bromus distachyos*, spicis duabus erectis alter-  
nis. Lin. Sp. pl. ed. II. 115.
- Gramen spica brizæ minus. Bauh. pin. 9.  
Pluk. Phyt. t. 33. f. 1.
- 2223 *Cynosurus cristatus*, bracteis pinnatifidis. Lin.  
Sp. pl. ed. II. 105. Hudf. Fl. Angl. 47.
- Gramen cristatum. Bauh. hist. II. 468. R.  
Syn. 398.

- 2224 *Cynofurus echinatus*, bracteis pinnato-paleaceis  
aristatis. Roy. Lugdb. 64. Lin. Sp. pl.  
ed. II. 105. Hudf. Fl. Angl. 47.  
Gramen alopecuroides spica aspera. Bauh.  
pin. 4.
- 2225 *Cynofurus aureus*, paniculae spiculis sterilibus  
pendulis ternatis, floribus aristatis. Lin. Sp.  
pl. ed. II. 107.  
Gramen panicula pendula aurea. Bauh. pin. 3.  
theatr. 33. Scheuch Agr. 149.
- 2226 *Festuca elatior*, panicula secunda erecta, spi-  
culis linearibus muticis, floris planis. Hudf.  
Fl. Angl. 37.  
Gramen loliaceum, spica divisa, pratense majus.  
Hist. Ox. III. 184. t. 2. f. 15.
- 2227 *Festuca pratensis*, panicula secunda erecta,  
spiculis linearibus muticis, floris planis.  
Hudf. Fl. Angl. 37.  
Gramen paniculatum elatius, spicis longis mu-  
ticis et squamosis. R. S. 411.
- 2228 *Festuca fluitans*, panicula ramosa erecta, spi-  
culis subseffilibus teretibus muticis. Lin.  
Sp. pl. ed. II. 111. Hudf. Fl. Angl. 38.  
Gramen aquaticum cum longissima panicula.  
Bauh. hist. II. 490. R. S. 412.
- 2229 *Festuca bromoides*, panicula secunda spiculis  
erectis laevibus calycis altera valvula inte-  
gra, altera aristata. Lin. Sp. pl. ed. II. 111.  
Hudf. Fl. Angl.  
Gramen paniculatum bromoides minus, pa-  
nicula aristatis unam partem spectantibus.  
R. Syn. 415.

- 2230 *Festuca ovina*, panicula secunda coarctata  
aristata, culmo tetragono nudiusculo, fo-  
liis selaceis. Lin. Sp. pl. ed. II. 108.  
Huds. Fl. Angl. 36.  
Gramen capillaceum, locustis pennatis non  
aristatis. Pluk. ph. t. 34. f. 2. R. Syn.  
410.
- 2231 *Holcus spicatus*, glumis bifloris muticis flo-  
ribus geminis penicillo involucriatis, spica  
ovato-oblonga. Lin. Sp. pl. ed. II.  
1483.  
Gramen paniceum sylvestre maximum, In-  
diæ orientalis. Pluk. Phyt. t. 32. f. 4.
- 2232 *Holcus Sorghum*, glumis villosis, feminibus  
aristatis. Lin. Sp. pl. ed. II. 1484.  
*Milium arundinaceum*, subrotundo semine,  
forgho nominatum. Bauh. pin. 26. Hist.  
Ox. III. 196. t. 5. f. 7.
- 2233 *Hordium murinum*, flosculis lateralibus mas-  
culis aristatis, involucriis intermediis ciliatis.  
Lin. Sp. pl. ed. II. 125.  
Gramen secalinum chalepense, radice tuberosa.  
Hist. Ox. III. 179. t. 6. f. 7.
- 2235 *Lolium temulentum*, spica aristata, spiculis  
compressis aristatis. Lin. Sp. pl. ed. II.  
122.  
*Lolium spica aristata*, radice annua. Lin. Sp.  
pl. 83. Huds. Fl. Angl. 44.  
Gramen loliaceum, spica longiore. Bauh.  
pin. 9. Theatr. 121.
- 2236 *Lolium perenne*, spica mutica, spiculis com-  
pressis multifloris. Lin. Sp. pl. ed. II. 122.  
P p p 2 Lolium



*Lolium spicis muticis*, radice perenne. Lin.

Sp. pl. 83. Hudf. Fl. Angl. 44.

*Gramen loliaceum angustiore folio et spica*.

Bauh. pin. 9. Theatr. 121.

2237 *Melica ciliata*, flosculis inferioris petalo exte-  
riore ciliato. Lin. Sp. pl. ed. II. 97.

*Gramen avenaceum montanum lanuginosum*.

Bauh. pin. 10. pr. 20.

2238 *Melica nutans*, petalis imberbibus, panicula  
nutante simplici. Lin. Sp. pl. ed. II. 98.  
Hudf. Fl. Angl. 31.

*Gramen avenaceum locustis rarioribus*. Bauh.  
pin. 10.

2239 *Panicum patens*, panicula oblonga patente, ca-  
lycibus bifloris, foliis lineari-lanceolatis.  
Lin. Sp. pl. ed. II. 86.

*Tifania-pullu*. Hort. Mal. XII. 75. t. 41.

2240 *Panicum miliaceum*, panicula laxa flaccida,  
foliorum vaginis pubescentibus. Lin. Sp.  
pl. ed. II. 86.

*Milium semine albo et luteo*. Bauh. pin.  
8. Theatr. 139.

2241 *Panicum verticillatum*, spica verticillata, ra-  
cemulis quaternis involucellis unifloris bi-  
fletis, culmo diffuso. Lin. Sp. pl. ed. II.  
82.

*Gramen paniceum spica simplici*. Bauh. pin.  
8. Theatr. 139.

2942 *Panicum viride*, spica tereti, involucellis bi-  
floris fasciculato-pilosis, seminibus nervosis.  
Lin. Sp. pl. ed. II. 83. Hudf. Fl. Angl.  
21.

**Gramen**

- Gramen paniceum, spica simplici. Bauh.  
pin. 8. Sch. Agr. 46.
- 2243 Panicum *colonum*, spicis alternis secundis muticis ovatis scabris, rachi teretiuscula. Lin.  
Sp. pl. ed. II. 84.
- Gramen paniceum minus spica divulsa infusæ  
Barbadensis. Pluk. Ph. t. 189. f. 5.
- 2244 Phalaris *canariensis*, panicula subovato spiciformi: glumis carinatis. Lin. Sp. pl.  
ed. II. 79.
- Phalaris major, semine albo. Bauh. pin.  
28.
- 2245 Phalaris *utriculata*, panicula spicata, petalis  
arista articulata, vagina supremi folii spathiformi. Lin. Sp. pl. ed. II. 80.
- Gramen pratense, spica purpurea ex utriculo  
prodeunte. Bauh. pin. 3. Theatr. 44.
- 2246 Phalaris *arundinacea*, panicula oblonga ventricosa. Lin. Sp. pl. ed. II. 80. Hudf.  
Fl. Angl. 21.
- Gramen arundinaceum spicatum. Bauh. pin.  
6. Theatr. 94.
- 2247 Poa *ciliaris*, panicula glomerata, glumarum  
valvulis interioribus piloso-ciliatis. Lin. Sp.  
pl. ed. II. 102.
- Gramen pratense, spicis brevibus muticis locustis minimis. Sloan. Hist. Jam. I. 114.  
t. 73. f. 1.
- 2248 Poa *Eragrostis*, panicula patente, pedicellis  
flexuosis, spiculis ferratis decemfloris. Lin.  
Sp. pl. ed. II. 100.

- Gramen phalaroides, sparsa brizæ panicula,  
min. Barr. Ic. 44. f. 2.
- 2249 Poa *rigida*, panicula lanceolata subramosa,  
floribus alternis secundis. Lin. Sp. pl. ed. II.  
102. Hudf. Fl. Angl. 35.
- Gramen exile duriusculum in muris et aridis  
proveniens. R. Syn. 410.
- 2250 Poa *compressa*, panicula secunda coarctata,  
culmo obliquo compresso. Lin. Sp. pl.  
ed. II. 101. Hudf. Fl. Angl. 33.
- Gramen paniculatum, radice repente, cul-  
mo compresso. Vaill. parif. 91. t. 18.  
f. 5.

Received November 12, 1767.

XLIX. *An Account of some neutral Salts made with vegetable Acids, and with the Salt of Amber; which shews that vegetable Acids differ from one another; and that the Salt of Amber is an Acid of a particular kind, and not the same with that of Sea Salt, or of Vitriol, as alledged by many chemical authors.* By Donald Monro, M. D. Physician to His Majesty's Army, and to Saint George's Hospital.  
F. R. S.

Title read December 17, 1767.

Read January 14, 21,  
and 28, 1768.

**T**HOUGH no substances have been more generally used, both for the preservation of health, and the cure of diseases, than vegetable acids, yet hitherto they have been examined with so little care, that it has been the common received opinion, that they were all nearly of the same nature, at least as to their chemical properties, and possessed nearly of the same virtues; but the following account of neutral salts, made with these acids, and the fossil or mineral alkali, shews that they differ materially from one another.

Previous, however, to entering into the account of these salts, it will be proper to mention some few things relative to salts in general.

Simple salts are commonly divided into *acid* and *alkaline*.

The *acid* are reckoned four in number.

1. The vitriolic.
2. The nitrous.
3. The marine, or muriatic.
4. And the vegetable.

The *alkaline* three.

1. The vegetable, or that which is got from the ashes of most vegetable substances.
2. The fossil, or mineral, called likewise soda and natrum, which is got either by burning certain marine plants; or from sea salt; or in the bowels of the earth.
3. The volatile, which is got either by putrefaction, or by the force of fire, from most animal substances; or by distillation from mustard seed, and some other particular vegetables.

The *acid* are distinguished from each other, by their taste, smell, and other properties, but principally by their forming different neutral salts with the same alkali.

And the *alkaline* are known likewise from one another, by their forming different neutral salts, when joined with the same acid.

Hence when we find *acid*, or *alkaline* salts, in different bodies, if we saturate each with the same alkali, or

with the same acid, according as the original salt is of an acid, or of an alkaline nature, and find upon dissolving, evaporating, and crystallising the neutral salts, that they are all of the same kind, we conclude, that the original acid, or alkali, was the same in all; but if we obtain different neutral salts from each, we conclude that the original acid, or alkali, was different in each.

If there are no more alkaline salts in nature, than the three already mentioned; and if there were no more acids than four; then the number of neutral salts would be confined to the twelve marked in Dr. Cullen's Table; but it will appear from the following experiments, that instead of one, there are many vegetable acids; and that, therefore, the number of true neutral salts must be greatly multiplied \*.

\* Dr. Cullen's Table of neutral Salts.

Acid	Alkaline	Neutral Salt	Acid	Alkaline	Neutral Salt
Vitriolic	Vegetable	Vitriolic Tartar	Marine or Muriatic	Vegetable	Sal. digest. Sylvii
	Fossil	Glauber Salt		Fossil	Common Salt
	Volatile	Vitriol. Ammoniac		Volatile	Common Ammoniac
Nitrous	Vegetable	Common Nitre	Vegetable	Vegetable	Regenerated Tartar
	Fossil	Cubic Nitre		Fossil	Rochelle Salt
	Volatile	Nitrous Ammoniac		Volatile	Spiritus Mindereri

Dr. Vogel, Professor of chemistry in the university of Göttingen, in his *Institutiones Chemicæ*, published in 1752, gives a table of neutral salts, which comprehends the twelve mentioned by Dr. Cullen, with the addition of three or four more. He seems to believe, that the acid of vitriol forms a different neutral salt with the pot-ash, and with the alkali procured from nitre. He mentions three salts, made with the vegetable alkali

Many chemists have affirmed, that the vitriolic is the only original acid in nature; and that the nitrous and marine are only this acid changed into different forms by foreign mixtures; and Dr. Boerhaave \*, Vogel †, Macquer ‡, and most late chemists, seem to think, that as all trees, plants, and other vegetable substances, receive their nourishment from the bowels of the earth, therefore their acids are only some of the mineral changed into a different form by the vegetative process; and that they all approached in their nature either to the vitriolic, the nitrous, or the marine: and as the neutral salts, produced from the mixture of the vegetable alkali, with vinegar, cream of tartar, and other common vegetable acids, have a good deal of the same external appearance, most chemists have concluded, that all vegetable acids were nearly of the same nature; though some few have suspected, that they might be found to differ from one another, and to have different degrees of affinity, if they were examined with care; and to confirm this, Dr. Vogel § tells us, that if some of the Rochelle salts be thrown into a decoction of tamarinds, the alkaline basis of the Rochelle salt will unite with the acid of the tamarinds, and the cream of tartar will be precipitated.

and vegetable acids; to wit, with vinegar, crystals of tartar, and lemon; and one with the native acid salt (as he calls it) of urine, and the volatile alkali, and one with the acid of tartar, and the volatile alkali.

\* Boerhavi Element. Chemiæ, vol. I. 804.

† Vogel, Institut. Chemiæ, p. 215. sect. 468. Ed. II.

‡ Macquer, Elemens de Chymie Theorique, chap. xvi. p. 240.

§ Vogel, Institut. Chemiæ, p. 216. sect. 469.

As I always suspected, from the taste and smell that vegetable acids differed materially from one another, and was the more confirmed in this opinion by the above experiment mentioned by Dr. Vogel, I began to consider whether some method might not be fallen upon to determine this question; and, on recollecting, that the Rochelle salt \* con- creted into large \* solid crystals, which preserved their figure long, even in the open air, though the tartarus tartarificatus † always appeared in the form of a powdery foliated salt, and run very soon *per deli- quium*, when exposed to the air, I imagined, that if we were to unite the fossil, or mineral alkali, with different vegetable acids, we should be able to ob- tain true neutral salts in form of regular crystals; which would shew how far these acids differed or approached to each other in their nature and proper- ties; and, upon trial, found that I had judged right; for each particular acid almost yielded a neutral salt pecauliar to itself, of which I shall now give a particu- lar account, and shall range these salts under the following heads.

1. Of neutral salts formed with native vegetable acids.

2. Of neutral salts formed with fermented vege- table acids.

\* The Rochelle salt is made with the crystals of tartar, and the fossil alkali.

† The tartarus tartarificatus, with the crystals of tartar, and the salt of tartar; so that the only difference between these two salts is, that the one is made with the fossil, and the other with the vegetable alkali.



3. Of neutral salts formed with distilled vegetable acids.

4. Of neutral salts formed with flowers of benzoin and salt of amber.

## S E C T. I.

*Of neutral salts formed with native vegetable acids, and the fossil or mineral alkali.*

### EXPERIMENT I.

With the acid of lemons.

The first experiment I made was with the acid of lemons; six ounces of the juice saturated rather more than three drams of the fossil alkali; and upon evaporating the liquor to a pellicle, and letting it stand for some days, I obtained a salt composed of a number of small crystals of irregular figures; some appeared to be irregular squares, or rhomboidal; others irregular pentagons; others to have more sides; but this general appearance was nearly what is represented at *a. a. a.* &c. in TAB. XXIII. fig. 1. They were mostly flat, and not above  $\frac{1}{4}$  or  $\frac{1}{2}$  of an inch thick; though some few were somewhat of an oblong irregular cubical shape, if I may be allowed to use the expression.

Having observed that the figure of neutral salts, made with vegetable acids, varied sometimes, according as they were crystallised in larger or less quantity, I got a quart of lemon juice, and saturated it with about two ounces, and two drams of the fossil

fossil alkali. Before adding the alkaline salt in this experiment, I tried the temperature of the lemon juice with one of Fahrenheit's thermometers, and found that the quicksilver stood in the tube at 54; upon removing the thermometer, I immediately added the alkaline salt; and as the solution was begun, I again put the thermometer into the liquor, and let it remain for above a minute, and the quicksilver sunk above one degree; so that this acid generates cold in the time of its uniting with the fossil alkali, though the neutral salt, produced from their union, does not affect the thermometer in the time of its solution in water.

The appearance of the salt obtained in this crystallisation was very different from what it was in the former. The whole was made up of an infinite number of crystals, so small that one at first could scarce distinguish their figure; but on examination part seemed to be of the same shape as the larger ones, got in the former process; the others were very small oblong parallelograms, and they were every where interspersed with a number of small longish crystals, which in many places lay across each other, and formed a kind of lattice work. The general appearance of this crystallisation is represented by *b. b.* &c. in Tab. XXIII. fig. 1. and that of some of the particular crystals by *c. c. c.*

The taste of this salt is very mild, and rather pleasant, approaching a little to that of a very weak sea salt.

## EXPERIMENT II.

With the acid of limes.

The lime is a fruit of the same genus as the lemon; its acid is sharper, and has a more agreeable flavour. From the near resemblance of these two fruits, one should have suspected that the neutral salt of both would have been almost the same; but their appearance is somewhat different, though perhaps upon more accurate trials they may be found to have nearly the same virtues, and chemical properties.

The first experiment I made was with the juice of a dozen and a half of small limes; and the neutral salt, produced from thence, was of the same shape, figure, and appearance, as the larger crystals obtained in the first experiment with the lemons; only the crystals were much smaller, and such as represented by *a. a.* &c. fig. 2. But having afterwards procured three dozen of larger and finer limes, I got from them near three times the quantity of juice I had in the former process; and having saturated this with the alkali, evaporated and crystallised it, I obtained a salt very different in its appearance from the former; though, in other respects, it seemed to be intirely of the same nature. Its crystals were of the size, and somewhat of the appearance, of barley corns, or grains of wheat, as at *c. c. c.*; some larger, some smaller; and laid in an irregular manner, but so as to form a beautiful crystallisation, which is represented by *b. b. b.* fig. 2. They appeared,

peared, at a little distance, to be roundish, but on examining narrowly, their sides were found to be made up of five or six flat surfaces; and generally one end of each crystal was made up of two flattish sides, which rose like a wedge which did not come quite to a point, but left a small narrow surface between.

These crystals, in the mouth, impress at first a very slight saltish, and somewhat sweetish cool taste; which is by no means unpleasant, and resembles a good deal that of the salt of lemons. They did not affect the thermometer in the time of their solution in water.

#### EXPERIMENTS III. and IV.

With the acid of Seville oranges, and of peaches.

It being late in the summer before I made any experiment with the juice of the Seville oranges, I could get none of this fruit but what had been long kept and was shrivelled, in so much that a dozen and a half of the oranges did not yield more than half a pint of juice, which had lost a great deal of its acidity, and saturated but a very small quantity of the alkali; and on crystallising I could obtain no other salt but a few very small cubical or square crystals, such as are represented by fig. 3. and similar to the salt got in an experiment I made with peaches, as may be seen in fig. 4. A saponaceous or mucous matter, with which these saturated juices abounded, seemed to prevent the crystallisation of the salts.

As both the juice of the orange, and of the peaches, was in small quantity, and not in the most proper state for yielding a neutral salt, these experiments ought to be repeated, before we can say what is the natural figure and appearance of the salts, that may be got by saturating the juices of these fruits with an alkali.

#### EXPERIMENT V.

With the acid of currants.

A quart of the juice of white currants, after being saturated with about nine drams of the fossil alkali, and purified by repeated filtrations, was evaporated till a pellicle appeared; being put into a cool place, and allowed to stand for two or three days, it yielded a number of small square flattish crystals, such as are represented by fig. 5. Many of them seemed to be exact squares, and in general they approached nearer to this figure, than the crystals of any of the other neutral salts I have hitherto met with.

This salt approached in its taste to that of the limes; its crystals were hard and firm, and did not run *per deliquium*.

#### EXPERIMENT VI.

With the acid of gooseberries.

A quart of the juice of gooseberries, being treated in the same way, as that of the currants, yielded a  
neutral

neutral salt very different in its appearance, from any of those hitherto mentioned. Its basis, or what adhered to the tea-cup, was made up of a number of very small roundish or squarish crystals; which formed an incrustation thicker than a shilling; from which grew up a number of very fine, thin, transparent plates, of irregular shapes; they were narrower at the basis than above; and in some measure might be compared to the scales of a small fish, or the wings of flies, set on their edges at a little distance from one another; in some places the plates arose from the sides of others; and in others they appeared somewhat like the fine leaves of very small plants. In fig. 6. we have different views of this salt; *a. a.* represents a piece of crystallised salt viewed from above; *b. b.* a profile view of the thin plates standing on their basis; *c. c.* the basis itself; and the letters *d.* a view of the thin plates laid on their flat sides.

### EXPERIMENT VII.

With the acid of apples.

Having got two dozen of codling apples, I cut them to pieces, put them into a large earthen vessel, and poured three quarts of water upon them; and then dissolved above two ounces of the fossil alkaline salt in the water, and let them stand for six days; on examining, I found the water to be nearly in a neutral state; it did not ferment on the addition either of an acid or of an alkali. I then filtered the liquor through paper, and evaporated it,

till it was reduced to about five or six ounces, when it became thick, and a pellicle began to form on the surface. I then set it in a cool place, to allow the salts to concrete. After two days were elapsed, it was covered with a blueish variegated saline crust; immediately below which was a clay coloured saline matter, which resembled wet earth or sand, that had been raised by small worms; and this was interspersed every where with small flattish globules of the same sort of matter; below this was a purplish jelly, interspersed with a whitish or ash coloured saline matter, formed into irregular longish flat plates, which looked more like a composition of salt and earth than a pure salt. The appearance of this saline matter made me suspect, that it was mixed with some sort of oil, which the alkaline salt had extracted from the skins of the apples, which I had forgot to peel off before infusing them in water; I therefore got a fresh parcel of codling apples, which I caused to be carefully peeled, and then treated them in the same manner as the former, and obtained the beautiful salt painted in fig. 7. which resembled a good deal the salt of the gooseberries; being composed of a number of small roundish very delicate transparent plates, standing on one edge, on a fine saline crust, which adhered every where to the sides of the china basin; and were interspersed with a grey coloured saline matter. The crystals of this salt were in general rather smaller, rounder, and more of a size, than those of the gooseberry; and I did not observe any rising from the sides of others as in it; and they seemed to be disposed in a more regular uniform manner.

The letters *a. a.* &c. represent pieces of this crystallisation viewed from above; *b. b.* &c. some of the fine plates laid on their side; *c. c.* and *e* some of the ash coloured plates obtained in the first operation; *d. d.* some of the brown clay coloured saline matter; *f.* the flattish globules, which beset every where the inside of the pellicle, that was on the top.

After the crystallisation of the salt in this second process, the liquor which remained was poured into another small china basin; and, on being evaporated, exhibited nearly the same appearances as had been observed in the first process.

## EXPERIMENT VIII.

With the acid of wild sorrel.

In order to save the trouble of a tedious evaporation, by saturating this acid mixed with the other juices of the plant, I procured some of the essential salt of the wild sorrel, from Mr. Heineken, apothecary in Duke-street; which I dissolved in boiling water, and saturated with the alkali; and by evaporating obtained a beautiful pure white neutral salt, which is represented by fig. 8; *a. a.* shews a part of the crystallisation where the salt shot into longish crystals, resembling somewhat the small ones of nitre; none of them exceeded the length of half an inch; *b. b.* other pieces of the crystallisation, which had a different appearance; *c. c.* a piece where it appeared like a small granulated salt; *d. d.* some small roundish or square crystals, which adhered like



a crust to the sides of the tea-cup; *e. e. e.* detached crystals.

### EXPERIMENT IX.

With the acid of tamarinds.

Having had a present of some tamarinds in pods, from Mr. Arch. Gloster, practitioner in physick in Antigua, I took out the pulp, and put about two pound of it into three quarts of water; and then saturated its acid with the alkali, and, after filtrating the liquor, I evaporated it to the consistence of a syrup, and then put it into a cool place for 24 hours; when I found that a crystallisation had actually taken place, I separated the salt from a thick sweetish liquor of the consistence of a syrup; after it was dried, it had the appearance of a piece of common moss, made up of a number of small crystals disposed in an irregular manner, and mixed with viscid or saccharine juices. The letters *a. a.* &c. of fig. 9. shew some pieces of this salt while it remained in this form.

As I suspected this salt to be still mixed with a viscid matter, I dissolved some of it in warm water, and crystallised it anew, when it had a very different appearance, for it had shot into an infinite number of very small crystals, which came every where from centres. The length of these crystals did not exceed half an inch at most; they were no thicker than horsehairs, or common white thread. How many crystals shot from each centre I could not determine; but, in many places, the crystallisation

sation rose into small oblong, oval, or roundish tufts, made up of an infinite number of the small crystals that shot from the centre towards the circumference.

Some of the concentrated liquor having been accidentally left in a faucer, and on the sides of a tea cup at night, next morning the liquor in both vessels had shot every where into small fine crystals, that came like radii from a centre; in some places they had compleated the circle, in others only half, and in others only the two opposite quarters.

I treated three pound of East-India tamarinds, which I bought in a shop in Castle-Street, in the same manner; only, after they were saturated with the alkali, and the liquor filtrated, it was set by for some weeks, and then filtered again before it was evaporated. The people of whom I bought these tamarinds told me, that there was no sugar mixed with them; and I believe what they said was true, for I obtained easily a very pure and fine salt from them. Having at first carried the evaporation too far, as soon as the liquor was removed from the fire, it immediately began to concrete in form of a number of small circles on the surface of the liquor, which were successively succeeded by others, till the whole became one solid mass; but on dissolving this mass in water, and evaporating only a little, and setting the liquor in a cool place to allow the salts to concrete, the crystallisation began on the surface of the liquor, in form of small circles or stars, and I obtained a salt in every respect similar to the former.

The general appearance of this salt is represented by *b, b*, in fig. 9, and the different appearances in different parts of the crystallisation by *c, c*.

EXPER-

## EXPERIMENT X.

With the acid of plums.

Having got a quantity of the larger sort of the green plums, I caused the stones to be picked out, and the plums to be bruised and put into a large china bowl; I then poured about two quarts of water over them, and saturated their acid with the fossil alkali; after they had stood 24 hours, I strained off the liquor, filtered, and evaporated it, till there remained only a few ounces, when it was set in a cool place for four days, at the end of which time I found that a crystallisation had taken place; but, upon pouring off the superfluous liquor, I could not observe any general form of crystallisation; the whole was made up of a number of very thin, flat, longish crystals, from about  $\frac{1}{12}$  to  $\frac{3}{4}$  of an inch long, and from  $\frac{1}{12}$  to  $\frac{1}{6}$  or  $\frac{2}{3}$  of an inch broad, of an irregular figure, laid without any particular form, and mixed every where with a mucous and black oily matter; when dry, the whole appeared like a confused mass, where however the form of some of the crystals was to be observed, as is to be seen at *a, a*, in fig. 10; in some places the crystals seemed to be laid with their edges uppermost, and in others in a different manner, as at *b, b*, &c.

In order to know the regular and true form of the crystallisation of this salt, I separated a quantity of the purest from the large mass, dissolved it in boiling water, filtered it through paper, and crystallised it a second time in a tea cup. It now appeared in a more regular form; the crystallisation

tion was divided into four roundish clumps, or clusters, which were separated or distinguished from each other by a furrow. Each clump was made up of a number of very fine delicate plates laid edgeways, in somewhat of a regular manner; and between them a number of others, where part of the flat sides were to be seen, and amongst them an infinite number of small rhomboidal or roundish crystals; the clumps appeared, in miniature, in the sun, somewhat like to the lower part of the spread tail of a peacock. The letters *c, c, c, c*, shew the general form of the crystallisation, *d, d, d*, the form and shape of some of the separate crystals.

I treated another parcel of plums of the same kind in a different manner; I saturated part of the liquor when fresh, and let it stand till a fermentation had taken place before I evaporated it; and I let the other half stand till the fermentation was over before saturating it; but the salt obtained from both, appeared nearly in the same form, though the number of clumps in this second crystallisation was only three: and in a third experiment the appearances were exactly similar, only the salt did not divide into clumps.

This salt tastes coolish on the tongue, but does not affect the thermometer, in the time of its solution in water.

In the present hard frost \* some of the salt of plums, which had been dissolved in three or four times its own weight of water, and set by in a closet, crystallised anew. The crystals were flat, thicker than a shilling, and most of them had six sides of unequal

\* N. B. The account of this last part of the experiment was given in to the Royal Society in the beginning of January 1768.  
lengths,

lengths, as represented by *b, b*, in the figure marked large crystals of neutral salt of plums: where they were run together, their figures were most irregular as at *i, i*; some few small ones were squares, as at *k, k*. Hence we see what a variety the different methods of crystallisation make in the figures of these salts.

## EXPERIMENT XI.

With the acid of mulberries.

Three quarts of the pure juice of the mulberry being saturated with four ounces of the fossil alkali, filtered, clarified with the whites of eggs and evaporated, yielded a saline matter, mixed with a quantity of a mucus and oil; which, on being purified as much as possible, by lying on a spongy brown paper, exhibited a very fine granulated salt almost like sea sand, in which no regular-formed crystals were to be observed.

Some of this salt being again dissolved in water, evaporated and crystallised a second time, appeared in the tea cup like a cake made up of the same sort of fine granulated salt as represented at *a, a*, fig. 11;—and another parcel *b, b*, which was treated in the same manner, though it at first appeared rougher, and more of a crystalline form, yet on examination was found to be made up of the same sort of saline matter.—On taking out the cake, there were a few thin very small square crystals, such as those marked *c, c, c*, adhering to the bottom of the tea cup.

A small quantity of the saturated juice of the mulberries having been left by accident for ten or twelve

twelve days in different tea cups and china basons, there formed in each a number of figures, or sort of crystals, resembling somewhat the alphabet of the Chinese language, interspersed with a few small, oblong, parallelogram-shaped crystals, as at *d, d, d*.

Three quarts more being saturated with the alkali, were allowed to stand for five or six weeks, and then filtered and evaporated to about seven or eight ounces; the greater part of which was put into a stone basin, and about half an ounce into a small china bowl; after ten days, the liquor in the bowl had shot into a number of small thin crystals, such as represented at *e, e, e*;—and that in the stone basin into a fine pure salt made up of similar crystals, but thicker and larger, such as those at *f, f, f*.—These last are certainly the true last crystals of this salt.

## EXPERIMENT XII.

With the acid of grapes.

A basket of grapes, which were brought to market for ripe, though many of them were still hard and sour, and not come to their full perfection, yielded three quarts of juice, which I filtered and saturated with two ounces and a dram of the fossil alkali:—after it had stood for a month to depurate, it was again filtered, and then clarified with the whites of eggs, and evaporated to about five ounces, when it became of the consistence of a syrup, and had somewhat of a sweetish taste.

It was then set in a cool place for two days to crystallise; but, instead of forming any regular crystals, it concreted in form of a saline matter,

resembling coarse loaf sugar, when it first concretes in a thick syrup; some of which being dried on brown spongy paper, became very white, and seemed to be made up of long, very small crystals, no thicker than human hairs, and a saccharine matter.

From this appearance, I judged that the salt was still mixed with a quantity of viscid juices; and therefore I diluted the whole with a quart of New River water, depurated it again with the whites of eggs, and evaporated it to four ounces, which I set in a cool place for eight days; and then, on examining, I found that a crystallisation had taken place, and I obtained above a dram of a pure neutral salt, made up of small, square, and cubic, and small narrow oblong parallelogram crystals, resembling somewhat in appearance those got from the juice of the mulberry, only the crystals were less, as may be seen in fig. 12.

After separating this salt, I set the remaining liquor again in a sand heat, and evaporated about half an ounce of it, and then put it for some days into a cool place; and there formed a saline saccharine-like concretion, exactly similar to what was got in the first trial.

## SECTION II.

### *Of neutral Salts formed with fermented vegetable Acids, and the fossil Alkali.*

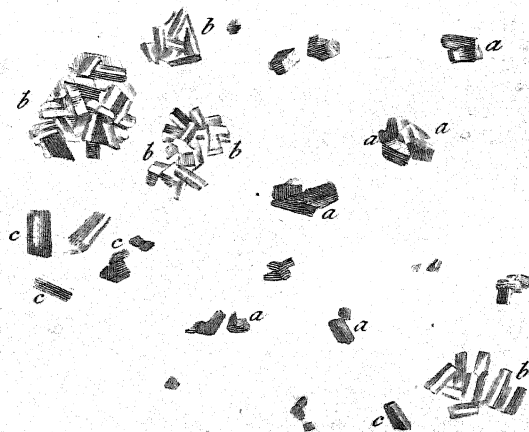
Having shewn a variety of neutral salts made with native vegetable acids, we come next to take a view of those made with fermented acids, and shall begin with that produced from vinegar.



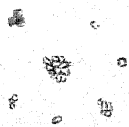




*N. Salt of Limes.*  
Fig. 2.



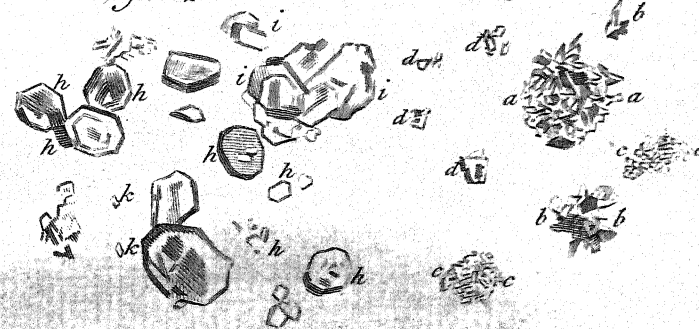
*N. Salt of Sevil Orange.*  
Fig. 3.



*N. Salt of Peaches.*  
Fig. 4.

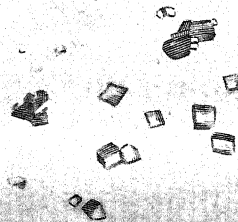


*Large Crystals of N. Salt of Plums.*  
Fig. 10. 2.

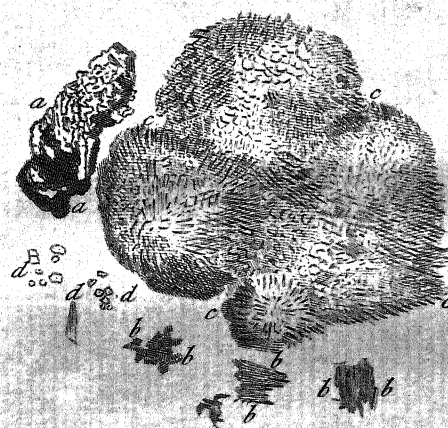


*N. Salt of Gooseberries.*  
Fig. 6.

*N. Salt of Currants.*  
Fig. 5.



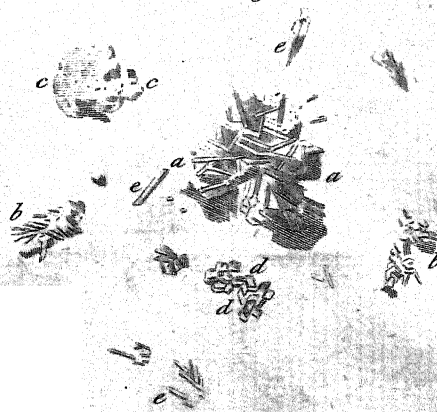
*N. Salt of Plums.*  
Fig. 10.



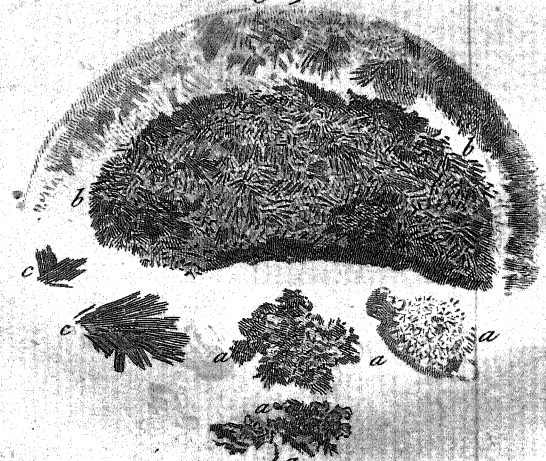
*N. Salt of Grapes.*  
Fig. 12.



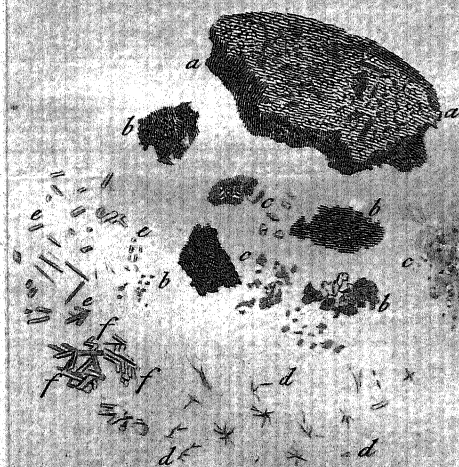
*N. Salt of Wild Sorrel.*  
Fig. 8.



*N. Salt of Tamarinds.*  
Fig. 9.



*N. Salt of Mulberries.*  
Fig. 11.





## EXPERIMENTS I. and II.

With common wine and distilled vinegar\*.

The plain vinegar used, was said to be the best white wine vinegar that could be got; and the distilled was said to be prepared from wine vinegar likewise.

I saturated a pint of each of these vinegars, with the pure alkali, evaporated them to a pellicle, and let them stand to crystallise.

From the distilled vinegar I obtained the salt represented by *a, a, a*, fig. 14, Plate XXIV. which in the evaporating glass appeared as you see it, resembling the figure of a sun in a fire-work. Its crystals were a little twisted; and upon taking them out, and examining them separately, they appeared like so many small crystals of glauber salt, as are represented by *b, b, b*.—On laying the evaporating glass on one side to allow the liquor to drain away, some thin flat square crystals, such as are represented by *c, c, c*, formed on the sides of the glass.

And what is very particular with regard to this salt is, that, on dissolving some of the crystals, resembling those of glauber salt, in pure water, and fully saturating the water with the salt, in some days there formed a number of crystals very different in figure and appearance from the former; be-

\* The neutral salt with vinegar I find mentioned, but not described, in a chemical dictionary published at Paris in the year 1766.—All that is said of it is, that it is a salt which crystallises easily, but is little known. See the articles ALKALI MINERAL, SEL NEUTRE, and VINAIGRE.

ing some of them squares, others longish parallelograms, others irregular pentagons, and some with six sides; some were flat, and from  $\frac{1}{17}$  to  $\frac{1}{5}$ , or  $\frac{1}{4}$  of an inch thick; others appeared somewhat roundish, or oval, but the sides were made up of flat surfaces, as are represented by the letters *d*, *d*, *d*, &c.

I do not remember to have seen such a variety of shapes and figures in any other salt; and I cannot account for the great difference of appearance in the first and second crystallisation in any other way, than from the liquor in the first crystallisation having been evaporated to a pellicle, and being very highly impregnated with the salt, the crystals began to shoot at once in every part of it, so that they had not room to extend in breadth, and to form themselves into various shapes, as in the second process, where the liquor was not so highly concentrated, and where each crystal was formed separate, and at a distance from another.

2. The plain wine vinegar, treated in the same way, but in a narrower vessel, yielded a salt which had a different appearance from the former; for on pouring off the superfluous liquor which remained after the crystallisation was completed, it seemed to be composed of a number of small, thin, broad crystalline, square plates, standing up from about half an inch to an inch above the surface, as represented by *a*, *a*, *a*, in fig. 13; on separating them, each crystal appeared at the basis like a small crystal of glauber salt, which terminated at the top in the thin broad plate already mentioned, as represented by the letters *b*, *b*, *b*, &c.

On dissolving some of this salt in water, and letting it stand for 18 or 20 days, there formed a  
number

number of crystals of different shapes, as in the distilled vinegar; some such as them, others resembling exactly the figure of the Rochelle salts, but smaller as those at *c*  $\times$ ; some squares, others of different shapes and figures, as at *c*, *c*, *c*, &c. and one large one marked *d*, *d*.

Both the salts from the distilled and from the plain wine vinegar, have a pleasant cool taste, without any disagreeable bitter; and generate cold in the time of their solution in water, for the quicksilver in the thermometer, which stood at 63 in New River water, sunk to 62, as soon as some of this salt, which was put into it, began to dissolve.

From the figure and shape of some of the crystals of the salt of the wine vinegar coming so near to that of the Rochelle salt, I think we may reasonably conclude that the acid of vinegar approaches near to that of tartar, but is not entirely the same.

The salt of the distilled vinegar can be made with great ease and very pure; but the common vinegar contains such a large proportion of oil as to require some care to purify it after it is made.

### EXPERIMENT III.

With the crystals of tartar.

The Rochelle salt, made with the acid of tartar, and the fossil alkali, is so common a purging salt, that I shall not enter into any description of it, and I have only given a figure of some of its crystals at fig. 15, that we might be able to compare it with

with the other neutral salts made with vegetable acids.

#### EXPERIMENT IV.

With the acid of verjuice of Apples.

I made two trials with the verjuice; the first was with a quart of what was old and tart, and took rather above an ounce of the alkali to saturate it; the other, which was newer, not so tart, and was saturated with about seven drams of the alkali.

The first was evaporated to a few ounces, when an oily or mucous pellicle appeared on the top; after letting it stand for some days, no salt was likely to crystallise; I therefore diluted it with water, filtered it, clarified it with the white of an egg, and evaporated it a second time; and after it had stood for some days the salt concreted into the form represented by fig. 16. It was composed of a number of small long crystals, which branched out from centres somewhat like the sticks of a fan, or the fibres in the leaf of a tree, such as at *a, a, a*.

The second or new verjuice, after being saturated, was allowed to stand for four or five weeks, then filtered, purified with the white of an egg, and evaporated; and after standing some days in a cool place, a crystallisation was formed, which approached very near in its appearance to the salt of apples, though somewhat different; it was composed of a number of very fine delicate small, flat square or rhomboidal plates set upon their edges, near to one another, without any certain regular order

order that I could observe, but so as on the whole to make a very beautiful appearance; in this crystallisation the salt seemed to form in clumps, two of which are to be seen at *b, b*, &c. and a profile view of a small piece at *c, c*, and a figure of some of the plates laid on their flat sides at *d, d*.

Did the difference of the age of the verjuice employed in these two experiments, or the difference of the processes they underwent, make the difference in the appearance of the salts obtained in the different crystallisations? The salt of the old verjuice approached to that of vinegar; of the new to that of apples.

#### EXPERIMENT V.

With the acid of perry.

At the time I gave in this paper, in the beginning of November, I had made several attempts to obtain a neutral salt from perry (or the fermented juice of pears) but without success, owing to the large quantity of saccharine juice with which this liquor abounds. But having accidentally left some of the concentrated liquor in a small china basin, on examining it some days after the present hard frost had begun, \* I found that a crystallisation had taken place.

The crystals were flat, long, narrow, very thin transparent plates, such as represented in fig. 17; they were from a quarter of an inch to near an

\* The account of this experiment was given to the Royal Society about the middle of January 1768.



inch long; they were mostly fixed to the sides of the bason by one end, many stood almost upright, and others lay across each other. One end was commonly made up of two short sides, which met at a point.

They remained some days exposed to the air in a cold room, and preserved their transparency and figure; but after they had stood for about a quarter of an hour in a warm room, while the painter was drawing the figure, they lost their transparency, and became white and mealy. They tasted cool, and somewhat bitter in the mouth.

### S E C T I O N III.

*Of neutral Salts, formed with distilled vegetable Acids, and the fossil Alkali.*

Acids distilled from wood, and other vegetable substances, have been mentioned as a distinct species, but no proof has been brought of their differing from the other vegetable acids; on the contrary, in the tables of neutral salts given by chemists, no notice is taken of any neutral salts made with these acids; and therefore it is to be presumed that they imagined them to be nearly of the same nature with the others.

In order to know if these acids differed from one another, and from the native and other acids, I had some guaiac wood, some fir wood, and some honey distilled, and procured some of the acid of each, which I saturated, filtered, evaporated, and crystallised.

# EXPERIMENT I.

With the acid of guaiac wood.

The crystals of the neutral salt of guaiac wood were long and small, and shot like the rays of the sun from a centre, and appeared as represented in fig. 18.

# EXPERIMENT II.

With the acid of fir wood.

The neutral salt of fir had a very different appearance; there were no such distinct crystals as in the other; what were to be observed seemed to be long and small, to come in many places from points, and to go in somewhat of a circular manner, or to describe a curve, and appeared as represented by fig. 19.

# EXPERIMENT III.

With the acid of honey.

It has been a doubt among naturalists, whether honey should be ranked among the vegetable or the animal substances. Most chemists seem to think it should be ranked among the vegetable, and look upon it as made up principally of the juices of plants collected by the bees; but, however that matter may be, the following is an exact account of

the neutral salt made with the acid obtained from this substance by distillation.

In order to procure this acid, I prevailed with Mr. Winter, brother-in-law to Mr. Heineken, apothecary, to distil four or five pounds of honey in a retort; at first he imagined that I only wanted the watry phlegm, which has been called by the name of the spirit of honey, and stopped the distillation before the acid came over; but having distilled a second quantity, he procured me about six ounces of a very acid liquor, which I mixed with the phlegm or spirit which he first brought me; I then saturated the whole with the fossil alkali, filtered and evaporated the liquor to a pellicle. After it had stood all night in a cool place, I found the pellicle to be composed of a yellow, bitter, saltish, mucous and oily matter; below which was a dark purplish liquor, which I poured into a tea cup, and there remained at the bottom of the stone gallypot, in which the evaporation had been performed, a yellow concreted matter, somewhat of the appearance of yellow wax, mixed with a little honey; on the surface of which was to be observed a number of globules of the same sort of matter, of the size of mustard seeds, and interspersed with a black very bitter stuff. Next day, on examining the dark coloured purplish liquor which I had put into the tea cup, I found that a great part of it had concreted into a very beautiful salt, which is represented by fig. 20. *a, a, a*, shews the general form of the crystallisation; *b, b, b, b*, the shape, figure, and size of some of the crystals. The crystals were almost all flat, and seemed in general to assume the form of  
long,

long, narrow parallelograms, or longish squares, if I may be allowed to use the expression; *c, c*, some of the yellow saline matter.

This salt is pleasant to the taste, and evidently generates cold in the mouth in the time of its solution; but I had not quantity enough to try with a thermometer what degree of cold it generated in the time of its solution in water.

## SECTION IV.

*Of neutral Salts formed with Flowers of Benzoin, and Salt of Amber.*

### EXPERIMENTS I. and II.

With the flowers of benzoin.

Most modern chemists have looked upon the gum benzoin as a resinous substance, which bears the same analogy to the vegetable resins, as the succinum or amber does to the fossil bitumens; and they have esteemed the flowers of benzoin to be an acid salt, mixed with an oily and a small proportion of an earthy matter; but have brought no proof of its being so.

1. In order to ascertain this fact, I put two drams and a half of the flowers of benzoin into some water, and then dropped into it by degrees a solution of the fossil alkali; every drop raised an ebullition or effervescence, in the same manner as when any common alkaline salt is thrown into an

acid liquor. I continued adding the alkaline lye till all ebullition ceased, and the flowers were fully saturated and dissolved; after which I filtered the liquor, and evaporated it till a pellicle began to appear, and then set it in a cool place all night, and next morning I had a fine pure transparent neutral salt, such as is represented by figure 21. It adhered to the china basin in form of a saline crust, which I removed; and on looking thorough it in the light, it seemed to be composed of an infinite number of very small crystals; above this lay, in many places, a number of crystals of the figure of small oblong parallelograms, as those at *b, b*. But from the greater part of the surface of the crust there arose a number of very fine thin delicate plates of irregular figures, standing on one edge; some were squares, others parallelograms, and others had more sides, the general appearance of which was such as is to be seen at the letters *a, a, a*, &c.

This salt, when first made, appeared as transparent and clear as glauber salt, or nitre; but on being exposed to the air, became very soon white and mealy.

In the time of the evaporation of this salt, a saline white mealy crust rose every where on the sides of the china basin in which the operation was performed, and even came over so far, as to cover its whole outside. What rose in this manner had a sweetish taste, and was not so sharp in the mouth as what appeared in a transparent saline form.

The superfluous liquor, which remained after the crystallisation was completed, being put into a  
tea

tea cup, concreted in a very uncommon manner. In the middle of the tea cup it arose something like a plant, or a fountain, where the water is discharged from a number of pipes, and spread from the bottom of this, so as to cover both the inside and outside of the cup, with a sweetish, white, mealy, saline crust, which in many places seemed disposed like the fine fibres of plants, or of the leaves of trees.

2. As a further proof of the flowers of benzoin being an acid of a particular kind, I saturated some of them with the sal volatile ammoniacum, evaporated and crystallised; and obtained an ammoniacal salt, which had a very singular appearance. It was covered on the top with a very white saline pellicle, below which were a number of thin, flat, white transparent crystals, the greater number of which seemed to be exact squares, some few, oblong parallelograms, such as are represented in fig. 22.

The flowers of benzoin generated a considerable degree of cold in the time of their saturation with the volatile alkali; they sunk the quicksilver in the thermometer from 52 to 46.

### EXPERIMENTS III. and IV.

With the salt of amber.

The salt of amber is now generally known to be of an acid nature; but from what Monsr. Bourdelin has said of it, in the Memoirs of the French Academy of sciences for the year 1742, its acid has  
2. been.

been looked upon by many chemists \*, to be exactly of the same nature as the spirit of sea salt, only mixed with a little of the oleum succini;—though some have imagined it to be an acid of the vitriolic kind.

1st, When I first mixed this acid with the fossil alkali, I began to believe that what Mons. Bourdelin had alledged was true; for the liquor tasted saltish, like to a weak solution of sea salt in common water, but I was soon convinced of my error; for on evaporating and crystallising, I had a salt very different in its nature and properties from that of sea salt, or of glauber salt, one of which salts it must have been †, had the acid been the marine or the vitriolic. This agrees with what Dr. Stockar de Neuform ‡, has said of this being a particular acid.

\* Macqner seems to be thoroughly convinced of the acid of succinum, or amber, being the same with that of sea salt; for in mentioning the proofs which Mons. Bourdelin has brought of its being so, he says, “C’est ce point qui est l’objet principal de mon memoire de Mons. Bourdelin; & cette decouverte est sans contredit une des plus belles, & en même temps des plus difficiles, qu’il y eût à faire sur ce Bitume.” See his *Elemens de Chymie pratique*, tom. ii. p. 213.

† Sea salt is a neutral salt made of the fossil alkali, and marine acid, or spirit of sea salt; and glauber salt, of the same alkali and the spirit of vitriol.

‡ In the year 1760, Dr. Jo. Geo. Stockar de Neuform, in his inaugural Dissertation de Succino, published at Leyden the 7th of July, 1760, proves by a number of experiments, that the acid of succinum is neither that of vitriol nor of sea salt; and he mentions two neutral salts made with this acid, the one with the common vegetable alkali, and the other with the volatile.

He says that the crystals of the one, made with the vegetable alkali, are clear and pellucid, and of the same figure as the

The

The crystals, I obtained in the first experiment I made, were large and flat; and such as are to be seen at the letters *a*, *a*, &c. fig. 23; they were of no certain shape or figure; some were roundish with a number of sides, others appeared somewhat triangular, and others of different figures; and in some parts the crystallisation appeared like a piece of rock work. I dissolved some of this salt in water, and crystallised it a second time, but the crystals were in general smaller than in the first operation; and the crystallisation appeared as represented by *b*, *b*, *b*. In order to shew the difference between this and sea salt, I made Mr. Paul draw the figure of some beautiful crystals of sea salt, near to those of this neutral salt of amber.

This salt is extremely different in its taste from that of sea salt, and certainly likewise in its virtues and properties.

2. In order to ascertain more fully that the sal succini is an acid *sui generis*, I saturated some of it with the volatile ammoniac salt, crystallised it, and obtained a neutral ammoniacal salt very different from that of the common sal ammoniacum; it was composed of a number of small long narrow flattish crystals, whose sides were made up of four flat surfaces, such as those represented in fig. 24, and laid

crystals of the salt of amber itself; that it has a particular taste, and dissolves easily in water, which the tartarus vitriolatus does not; and when thrown on the fire, or put on a red hot iron, crackles and melts, but yet remains fixed and neuter. Acids make no change on it, nor is aqua fortis converted into an aqua regia by its mixture; it does not precipitate silver from spirit of nitre, though it precipitates lead from vinegar, in form of a white calx, which, however, cannot be changed into a saturnus corneus.



in an irregular order, some lying across others, and some standing on one end \*.

The sal succini generates a great degree of cold in the time of its saturation with the volatile alkali, for it sunk the quicksilver in the thermometer from 52 to 40; in this it agrees with the common sal ammoniacum.

### The Conclusion.

From the experiments above related, it is evident that physicians have hitherto been in a great mistake, in believing that all vegetable acids were nearly of the same nature; for from them it should seem that almost each of the acids, called vegetable, has something peculiar to itself, and upon future trials may be found to have different virtues and properties †.

The different appearance of the neutral salts above mentioned, from that of those produced by the union of the fossil alkali with any of the mineral acids, seems to make it doubtful whether the vegetable acids derive their origin from the mine-

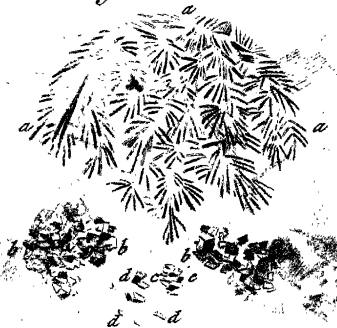
\* Dr. Stockar de Neuform says, that this ammoniacal salt does not precipitate silver from aqua fortis, nor change aqua fortis into aqua regia; and when put in a silver spoon, and set over the fire, it melts and flies off in form of a vapour.

† However, it ought to be remarked, that when any of the concentrated saturated liquors stood for ten or twelve days before they crystallised, for the most part some crystals of a flat, square, or of a narrow oblong parallelogram figure, were found adhering to the sides of the cup or basin in which the liquor stood; but whether this was owing to the alkaline basis of these salts, or to the acids approaching to each other in their nature, is what can only be determined by future experiments.

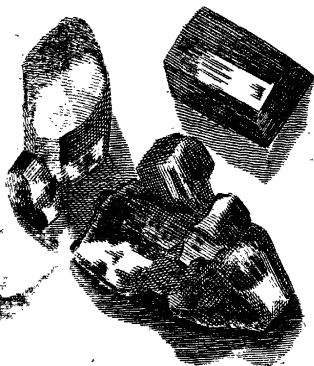
ral,



*N. Salt of Verjuice.*  
Fig. 16.



*Rochelle Salt.*  
Fig. 15.



*N. Salt of Guaiac Wood.*  
Fig. 18.



*N. Salt of Fir.*  
Fig. 19.



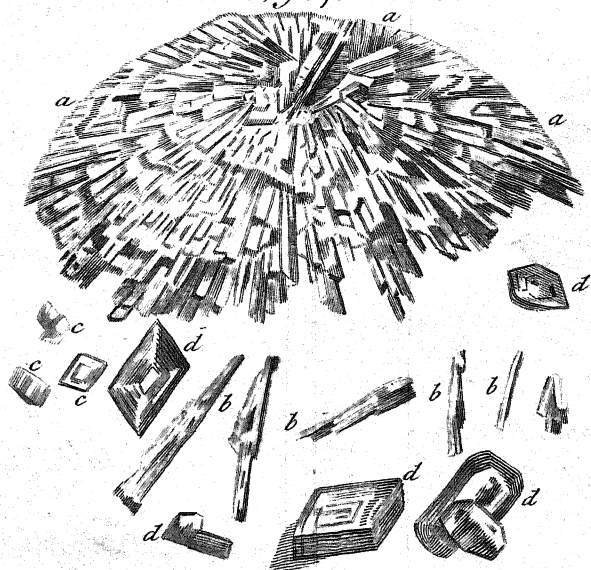
*N. Salt of Benzoin.*  
Fig. 21.



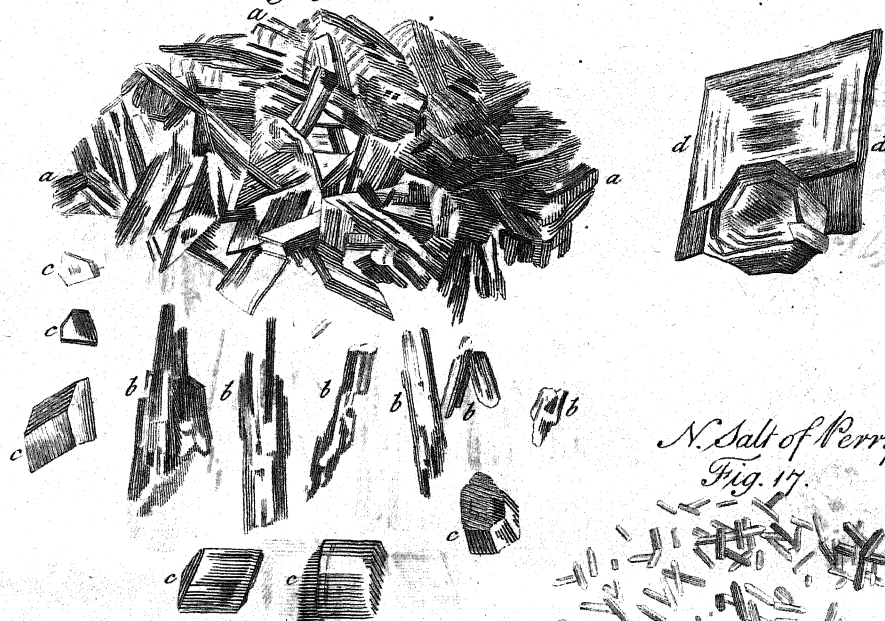
*Ammoniacal Salt of.*  
Fig. 22.



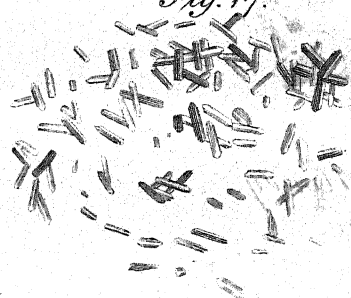
*N. Salt of Distilld Vinegar.*  
Fig. 14.



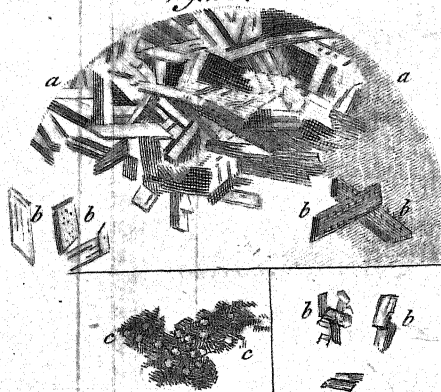
*N. Salt of Common Wine Vinegar.*  
Fig. 13.



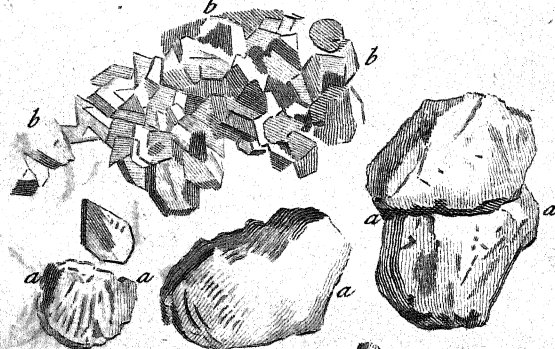
*N. Salt of Perry.*  
Fig. 17.



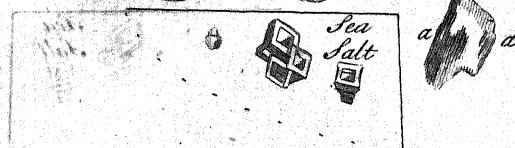
*N. Salt of Honey.*  
Fig. 20.



*N. Salt of Amber.*  
Fig. 23.



*Ammoniacal Salt of Benzoin.*  
Fig. 22.





ral; or whether they are not new substances, generated either in the vessels of plants by means of the vegetative process, or by fermentation, or by the force of fire. If they owe their origin to the mineral acids, they are certainly so much changed in their virtues, and properties by the combination of new particles, and by the processes they have undergone, that they may be looked upon as distinct bodies in many respects.

From what has been said, it is evident that the number of true neutral salts\* is infinitely greater than what has been supposed, of late, by chemists; and it is probable that many of the neutral salts, above described, may prove to be excellent remedies in the cure of diseases, as well as useful in many manufactories.

As there is such a variety of vegetable acids, and as each of them produces a distinct neutral salt with each of the three alkalies, I think it would be right to distinguish them from one another by particular names; the salts made with the vegetable alkali may be called *vegetable salts*, as both the acid and the alkali are vegetable substances; those made with the fossil alkali *neutral salts*; and those made with the volatile alkali *ammoniacal salts*, as all the neutral salts hitherto made with this alkali have gone by this name. Thus we may call the three neutral salts made with lemon juice: 1. Vegetable salt of lemons. 2. Neutral salt of lemons. 3. Ammoniacal salt of lemons.

\* By true neutral salt is meant, a salt made with an acid and one of the three alkalies; the word *true* is added to these salts, to distinguish them from neutral salts, made with earths or metals, and acids.

By means of these neutral salts we may be enabled to discover many of the properties of vegetable acids, and particularly the different degrees of affinity or attraction between them and alkaline salts; thus, for example, if we dissolve in water some of the neutral salt of currants, and add some lime juice or some vinegar, and then evaporate and crystallize: if we obtain a neutral salt of currants, we conclude that the acid of currants has a greater affinity or attraction to the alkali than the acid of limes or of vinegar; but, if we get a neutral salt of limes, or of vinegar, we conclude that these acids have a greater affinity with the alkali than the juice of currants.

As I am sensible that this account of vegetable neutral salts is very incomplete, and that I have done little more than given a very superficial description of their external appearance; and as it will probably require a length of time, and the labours of many, to discover fully their virtues and properties, I shall recommend it to those who may prosecute this subject to endeavour to ascertain the following facts:

1. What degree of cold or of heat is generated on the mixture of each acid with the different alkaline salts; and likewise to try the same experiment with each neutral salt at the time of its solution in water.

2. What quantity of pure alkaline salt it takes to saturate any determined quantity of each of the vegetable acids.

3. What figure each neutral salt assumes when it is first crystallised, and likewise after it has been purified;

purified, and again dissolved in water and crystallised.

4. What quantity of water it takes to dissolve any determined quantity of each salt.

5. What effects these salts or their solutions in water have on oils, sulphur, ardent spirits, metals, earths, and other substances; what substances they mix easily with, and to what bodies they prove a menstruum, or assist in dissolving.

6. How far they agree in their virtues and properties with the neutral salts made with mineral acids, and with each other.

7. What effects they have on the human body; whether they promote more particularly the perspiration or the secretion by the kidneys, or whether they act more readily on the bowels, and promote the discharge by stool; and to ascertain the exact and proper doses of each.

8. And lastly, what effects fermentation and distillation have on native vegetable acids; and to observe and compare the appearances of the neutral salts made with these acids in their different states: viz. 1. In their native state. 2. When made into wine. And 3dly, when made into vinegar; and likewise when made with acids brought over by the force of fire, or distilled from the same juices in each of the three different states mentioned.

And in order to facilitate their labours, I shall conclude this long paper with observing,

1st, That all vegetable juices used for making neutral salts ought to be strained through a cloth, and then filtered through paper, before they are saturated with the alkaline salt; and that, after they



are saturated, they ought to be allowed to stand for some days, and some of them for weeks, and then be filtered again, before they are evaporated.

2dly, That it is of use to clarify many of these juices, after being saturated, with the whites of eggs.

3dly, That it is sometimes easier to obtain a neutral salt, by evaporating with a boiling heat, than with a slow or gentle fire; as the heat of boiling water coagulates, and throws up a quantity of viscid juices to the surface, which cannot be easily separated by any other means.

4thly, That the sweeter any fruit is, and the more it abounds with saccharine or viscid juices, the more difficult it is to obtain a neutral salt; and for this reason I have not hitherto been able to get any neutral salt from the saturated juices of pears, or of cherries.

5thly, That, in cases where we are obliged to employ water mixed with the fruits cut small, instead of their juices, it is right to peel off the skins before we attempt to saturate the acid; otherwise the alkaline salt is in danger of uniting with, and rendering soluble in water, the gross oils with which the skins generally abound, which afterwards prevent the crystallisation of the neutral salts.

Received November 19, 1767.

L. *Experiments on the Distillation of Acids, volatile Alkalies, &c. shewing how they may be condensed without Loss, and how thereby we may avoid disagreeable and noxious Fumes: In a Letter from Mr. Peter Woulfe, F. R. S. to John Ellis, Esq, F. R. S.*

Title read December 17, 1767.

S I R,

Read Feb. 4,  
1768.

**I**N the common manner of distillation there escapes a great quantity of fumes, which cannot be condensed; and in several operations these fumes are very hurtful to the lungs. By the following method of distillation these fumes are totally condensed, which makes a great saving in some distillations, and the operator is in no danger of being hurt by any pernicious vapours.

This new method consists in making the fumes pass by a small glass tube through water, which hereby becomes charged with the vapours, that would otherwise escape.

Description

## Description of the apparatus, TAB. XXV.

Fig. 1.

A a retort.

B a receiver, with a spout at bottom, for the distilling liquor to run into the bottle C; the recipient has also a small opening on one side at D.

E a crooked tube  $\frac{1}{8}$  and  $\frac{1}{2}$  of an inch bore.

F a vessel containing water.

The crooked tube E is fitted to the spout D of the receiver by means of a cork with a hole in its middle, and then well covered with lute; the other end of it goes to the bottom of the vessel F, to the mouth of which it is fitted by a cork, with a semi-circular notch in it as at G, but without any lute to fasten it, as there must be a small vent for the escape of the elastic air, and this is the only vent in all the apparatus for that purpose. By this apparatus the fumes are obliged to pass through the water in F, and there deposit all they contain, except their elastic air.

In most distillations there is a quantity of air absorbed at different times during the process; and in this case the external air would press on the water at F, and force it by the tube into the vessel C, which might spoil the distilled liquor. This may be prevented by letting air into the receiver or bottle C, by boring a hole through the lute; this however may be inconvenient, on account of the constant attendance which is necessary; but the following apparatus will prevent it. See figure 2. It consists

consists in fitting an empty vessel H, to the apparatus described before, See figure 1. By this means the water is forced into H, and by the stopper at L it may be emptied, and put back into the vessel F, the crooked tubes D and I are fitted to H, by a cork with two opposite semicircular notches as at K, and then well covered with lute.

### EXPERIMENT I.

On the distillation of sal ammoniac with quick lime.

12 lb. \* of British sal ammoniac, and 26 lb. of quick lime were powdered, mixed, and put into the iron body A (fig. 3.); and when the apparatus † was luted, a gallon of water was poured on it through the orifice (b); which was immediately stopped; the lime growing hot produced a vast quantity of elastic air, which though highly charged with volatile alkaly was condensed by the water in F, fig. 2. the air only escaping at the top of this vessel with hardly any sensible volatile alkaline smell. Next morning, all being cold, another gallon of water was added as before, and a very slow fire made under the body for 14 hours, in which time there distilled near a pound of volatile alkaly; the fire was then made stronger, and continued in that state for twelve hours more, in which time there was obtained, together with what was first distilled, 8 lb.  $\frac{1}{2}$  of volatile alkaly, strong and fit for Eau de luce; this was taken out of the bottle and set apart. The vessels being cool,

\* In all the experiments averdupois weight was made use of.

† The spout of the stone head belonging to the body A, figure 3, is to be luted to the receiver B, figure 2.

two gallons more of water were put into the body, and the fire made as before, and continued till there was 7 lb distilled of weak volatile spirit; this answers better than water for a fresh distillation of sal ammoniac and lime.

During the first 16 hours of the distillation, there continually escaped through the water of F elastic air very slightly charged with volatile alkaly, especially when the water grew hot; but during the remaining time of the distillation, no elastic air was set free.

Two stone gallon bottles, with three quarts of water in each, were made use of to condense the vapours; and when one bottle was grown warm by the fumes, the other was put in its place, while it was a cooling in a vessel of cold water; and so continually changed during the whole operation. The six quarts of water encreased by this means 2 lb and  $\frac{1}{2}$  in weight; and, by the following experiments it appears, that a pound of this vapour condensed in the water is to a pound of the volatile alkaly, which was set apart for Eau de luce, as 140 to 76, which is nearly twice as strong; therefore there was a saving of near 1 lb of volatile alkaly, which would have been lost in the common manner of distillation.

The water of the two stone bottles charged with alkaline vapours was mixed, in order to reduce them to the same degree of strength, and as much of it was put into a glass cucurbit as contained four ounces of the alkaline vapour; four ounces of the volatile alkaly, which was set apart for Eau de luce was put into another cucurbit of the same size, and

and diluted with water to the same volume of the other.

This last took 1 lb.  $3\frac{3}{4}$  of acid of vitriol, diluted with water, to be saturated, and did not grow hot; whereas the water containing the four ounces of alkaline vapours took up 2 lb.  $3\frac{3}{4}$  of the same acid of vitriol, and grew so very hot, that the vessel could scarce be held in the hand, even after having been diluted at different times with two quarts of water. This shews that there is a great difference in the two, and that it is not intirely owing to strength.

The heat produced by the vapours passing through the water, was tried at another distillation, and raised the quicksilver in Fahrenheit's thermometer to 110 degrees.

In rectifying caustic volatile alkaly with lime, there is likewise a very great quantity of elastic air set free, highly charged with volatile alkaly, which condenses in water and heats it.

Water may be so strongly charged with this vapour, that it will make very strong Eau de luce, nay, much stronger than that which we said before was distilled and set apart for Eau de luce: but it is necessary, as mentioned before, to make use of two stone bottles, changing them as often as they grow warm.

## EXPERIMENT II.

On the distillation of the acid of salt by means of the acid of vitriol; for the apparatus see fig. 2.

\* A green quart retort coated with loam was made use of for this experiment; and it was placed

\* What goes by the name of a quart retort holds better than two gallons of water.

in a reverberatory furnace on a naked fire; 14 lb of common salt was put into it, and on that the like quantity of oil of vitriol, which had been diluted the day before with 7 lb of water; the retort was then immediately luted to the recipient, and the distillation conducted in the common manner: the operation continued 16 hours, when hardly any more liquor would come over with a strong fire.

To condense the vapours, two stone gallon bottles with three quarts of water in each were made use of, as in the former experiment.

In this operation there was obtained 9 lb 5  $\frac{3}{4}$  and  $\frac{1}{2}$  of spirit of salt, which dropped into the bottle C; the six quarts of water in the stone bottles increased in weight 6 lb 12  $\frac{3}{4}$  and  $\frac{1}{2}$ ; the caput mortuum weight 18 lb 6  $\frac{3}{4}$ ; so that in this operation there was only a loss of eight ounces, which is but  $\frac{1}{70}$  part of the whole, which probably was mostly elastic air.

### EXPERIMENT III.

The same operation was repeated with a flower fire, which continued for 23 hours, after which time hardly any more liquor would come over with a strong fire.

There were here produced 11 lb 10  $\frac{3}{4}$  of spirit of salt, in the bottle C; the six quarts of water increased in weight 3 lb 10  $\frac{3}{4}$ , and the caput mortuum weighed 19 lb 4  $\frac{3}{4}$ ; the loss was the same as in the foregoing experiment.

In order to know the different degrees of strength of the acids produced in these two experiments, they

they were saturated with a fixed alkaly dissolved in water.

Four ounces of the acid in experiment II, which distilled into C, took of the alkaline liquor to be saturated  $13\frac{3}{4} 53 2\text{D}$ .

As much of the water \* in experiment II, as contained  $4\frac{3}{4}$  of vapour took to be saturated  $1\text{lb } 9\frac{3}{4}$ .

Four ounces of the acid in experiment III, which dropped into C, took of the same alkaline liquor to be saturated  $12\frac{3}{4}\frac{1}{2}$ .

As much of the water of experiment III, as contained; four ounces of vapour † took to be saturated  $2\text{lb } 6\frac{3}{4}$ .

Four ounces of oil of vitriol, which was to water in weight as 24 to 13, took of the same alkaline liquor to saturate it  $1\text{lb } 10\frac{3}{4} 73$ , which shews that oil of vitriol is not so strong an acid as the vapour of spirit of salt, when condensed in water and distilled slowly, as in experiment III.

From the foregoing experiments it appears, that  $1\text{lb}$  of the spirit of salt vapour, condensed in the water in experiment II, is to  $1\text{lb}$  of the acid of salt, which dropped into C of the same experiment, as 200 is to 109, which is near double; and therefore the  $6\text{lb } 12\frac{3}{4}$  and  $\frac{1}{2}$  of the vapour, which condensed in the water, is equal very nearly to  $13\text{lb } 1\frac{3}{4}$  of the acid which is distilled in C: so that by this method of distillation, this great proportion of acid is saved, and those disagreeable suffocating fumes avoided.

\* The water of the two bottles was mixed together; for they were of different strength.

† The water of these two bottles were likewise mixed together for the same reason.



In experiment III, 1 lb of the acid vapours, which condensed in the water is to 1 lb of the acid of salt which dropped into C, as 131 is to 50, or as  $2\frac{3}{5}$  to 1; and therefore the 3 lb 10  $\frac{2}{3}$  of acid vapours, which condensed in the water, is almost equal to 9 lb and  $\frac{1}{2}$  of what distilled into C.

It further appears, that the slower the distillation is conducted, the more concentrated are the acid vapours that condense in water. In order to see whether there was any difference in the strength of the acid vapours, which were condensed in the water from the first to the last of the distillation, the following experiments were made.

Five pound of common salt, with 5 lb of oil of vitriol were distilled in a tubulated retort, and three bottles with an equal quantity of water in each were made use of to condense the vapours.

The first bottle increased in weight 3  $\frac{3}{4}$ , and during this time, which was twelve hours, there was no fire under the retort; that bottle being taken away, another bottle put under, a fire was made; this bottle increased in weight 1 lb and half an ounce, the third bottle increased 10  $\frac{2}{3}$  and a half.

As much water of each of the three bottles as contained one ounce and a half of the acid fumes was saturated with an alkaly dissolved in water.

The water of the first bottle took  
to be saturated

11  $\frac{3}{4}$  + 2 dram.

The second bottle took up

10  $\frac{3}{4}$  23. 20

The third bottle

10  $\frac{3}{4}$  13

An ounce and half of oil of vitriol, which was to water, as 226 to 118 nearly, took up of the same alkaly 73. 63.

By which it appears that the fumes, which first arose without fire, are stronger than the second, and the second than the third.

It appears further, that the most concentrated portion of the acid of sea salt is the most volatile, and that in strength it is to the oil of vitriol mentioned before, as  $44 \frac{1}{4}$  to 31.

\* In order to try the purity of the acid vapours, which were condensed in the water, and of the acid, which distilled into the bottle C, the following experiments were made, and are marked a, b, c, d.

(a) Four ounces of the spirit of salt of the 2d experiment, was perfectly saturated with  $4 \frac{3}{4}$  of whiting.

(b) Four ounces of the spirit of salt of the 3d experiment, was perfectly saturated with  $4 \frac{3}{4}$  of ditto.

(c) As much water as contained  $4 \frac{3}{4}$  of vapour of the 2d experiment, was saturated with  $5 \frac{3}{4}$  of ditto.

(d) As much water as contained  $4 \frac{3}{4}$  of vapour of the 3d experiment, was saturated with  $6 \frac{3}{4}$  of ditto.

The reason of using more whiting with some than with others, was on account of the different strength of the acids; and as there was a greater quantity of whiting than necessary used in these experiments to saturate the acids, the undissolved

\* This depends on the property of the acid of vitriol, and the acid of sea salt, combined with a calcareous earth; for this earth, combined with the acid of sea salt, forms a very soluble substance; whereas the same earth, with the acid of vitriol, forms a substance insoluble (or almost so), called selenite.

part must consist of whiting; and, if any acid of vitriol in the acids, of whiting and selenite.

In order to separate the selenite from the whiting, a large portion of distilled vinegar was made use of, which dissolves the whiting, it being a calcareous earth; and in order to promote the solution, heat was made use of.

The undissolved part of (a) being perfectly saturated with a sufficient quantity of distilled vinegar, and afterwards repeatedly washed with pure water, and dried, weighed  $\frac{1}{2}$  oz. and 26 gr.

(b) treated as (a) weighed  $\frac{1}{2}$  oz. and 52 gr.

(c) treated as (a) weighed 39 gr.

(d) treated as (a) weighed 42 gr.

\* One ounce of whiting treated as (a) left 7 gr.

From these experiments it appears, that the

Four ounces of acid marked (a) contain as much acid of vitriol as will make

$\frac{1}{2}$  an oz. less 2 gr. of selenite.

Four ounces of acid marked (b)

$\frac{1}{2}$  an oz. and 24 gr. of ditto.

Four ounces of the acid vapour marked (c)

4 gr. of ditto.

Four ounces of the acid vapour marked (d) none.

Hence it is evident, that the vapour of the acid of salt condensed in water, when distilled slow, contains no acid of vitriol; and that even when it is distilled quick, it contains so small a quantity as is not worth notice.

\* As whiting contains some parts which are not soluble in distilled vinegar, it was necessary to know how much of this an ounce contained, which must be deducted in proportion to the quantity used for the experiments a, b, c, and d.

If 10  $\frac{3}{4}$  of sea salt are distilled in the common manner with an equal quantity of oil of vitriol unmixed with water, there only distil 2  $\frac{3}{4}$  of spirit of salt; whereas, if distilled in this new manner, we not only obtain the like quantity, but likewise 4  $\frac{3}{4}$  and  $\frac{1}{2}$  half more, which are condensed in the water; so that in making this concentrated spirit of sea salt, there is a saving of above double the quantity, which would be lost in the common method of operating.

Of the heat produced by the vapours of spirit of salt passing through water, spirits of wine, and oil of turpentine:

Three quarts of water were put into a gallon stone bottle, and made use of to condense the vapours, as in experiment the 2d, fig. F; in three hours and a half after the fire was made under the retort, the water in the stone bottle had acquired the degree of 212, which is the mark of boiling water in Fahrenheit's thermometer; and at this time there was scarcely 2  $\frac{3}{4}$  of spirit of salt distilled into the bottle, fig. C. The receiver and bottle C seemed cold to the touch; the water at F had increased 215 3  $\frac{3}{4}$ . Another like bottle with the same quantity of water being put in the room of this, in some time, acquired the same degree of heat. The fumes seem to condense very well until the water acquired a heat within twelve degrees of boiling water.

Spirit of wine rectified, made use of instead of water to condense the vapours, acquires a heat equal to 188 degrees; and it grows of a deep brown colour, though transparent.

Oil of turpentine applied to the same use acquires a heat of 12 degrees above that of boiling water, or 224 degrees; it becomes of a dark brown colour, though transparent, and has a disagreeable bituminous smell. The thermometer not measuring more than 213 degrees, could not be left in with safety any longer.

Another time oil of turpentine was made use of to condense the vapours, which proceeded from 1 lb  $\frac{1}{2}$  of sal ammoniac, with 1 lb  $\frac{1}{2}$  of oil of vitriol, and  $\frac{3}{4}$  of a pound of water: here it did not grow near so hot, nor so high coloured, as in the other experiment, but was for the most part congealed.

The difference of these two experiments may, perhaps, be owing to the smallness of the quantity of the ingredients in the last process; for in the first there was 14 lb of salt, 14 lb of oil of vitriol, and 7 lb of water.

### Of the re-absorbtion of Air in Distillations.

In all distillations a quantity of elastic air is set free in the beginning, but afterwards there is a re-absorbtion of the same; the following experiment was made to shew how great it is in some cases.

For the apparatus, see fig. 1.

One pound and a half of foreign sal ammoniac was put into a retort, and 1 lb  $\frac{1}{2}$  of oil of vitriol (previously diluted the day before with  $\frac{3}{4}$  of a pound of water) poured on it, and a recipient well luted to it; the recipient had a tube 31 inches, well fitted and luted to it; and this tube was immersed in a glass vessel containing a quart of water.

The spirit of salt which was distilled,	lb	3	3
weighed		1	2 5
The quart of water increased in weight		5	4
The caput mortuum weigh'd	2	3	4
The loss in the operation was only			3

---

3 12

The operation was continued till the sal ammoniac began to sublime.

When no more air escaped, which might easily be perceived by its ceasing to bubble through the water, the vessel of water was taken away, and the tube was immersed in a basin of quicksilver; the mercury rose in the tube 23 inches and a half, whilst the recipient was too hot to bear one's hand on it longer than half a minute; when the recipient was quite cold, the mercury rose to 29 inches and  $\frac{1}{2}$ , and there was near one inch of spirit of salt on its surface. This experiment was tried the 11th of November, when the barometer was at 30 inches. In order to make this experiment succeed, it is of the utmost consequence to lute well the vessels.

### On the Marine Æther.

The Marquis De Courtenveau, of the Royal Academy of Sciences of Paris, has published a very curious memoir in their Transactions, on the making of Marine Æther, by distilling spirit of wine with the \* liquor fumans of Libavins; but no one, that I know of, has succeeded in making it with the pure spirit of salt. It was natural to conclude

\* The liquor fumans is made by distilling mercury sublimate with tin, and is composed of the acid of salt united with tin.

from the extreme great acidity of the fumes of spirit of salt, that *Æther* might be made by saturating rectified spirits of wine with them; and on trial I found it answer, though not in a large quantity.

The spirit of wine, charged with the acid vapours, must be distilled and cohobated, and then rectified with a slow degree of fire \*.

The method that *Monf. Beaumé* of Paris proposed to make this *Æther*, and which did not succeed with him on account of his not being able to condense the fumes, answered well with me; and it consists of combining the vapours of spirit of salt with those of spirit of wine. The apparatus that I made use of for this purpose is described at fig. 4, and the process is as follows:

Eight pound of sea salt was put into the retort B, and two quarts of rectified spirit of wine into the retort D; three pints of the same spirits of wine were put into each of the glass vessels I and K, in order to condense the fumes, one not being sufficient; all being well luted and secured, the spirits of wine in D were made to boil, and then 7 lb of oil of vitriol was poured on the salt in the retort B, at ten or twelve different times, seven minutes between each time, lest the mixture should boil over; then a fire was made under this retort, and both fires kept up till the operation was over. The quantity of liquor in the vessels I and K, increases considerably from the vapours that condense therein; and the vessel I in particular grows very hot, and being

\* As I have shewn before, that the vapours of the acid of salt, which condense in water, are free from the acid of vitriol, we may be certain, that the acid of vitriol did not contribute to form this *Æther*.

highly charged with vapour is rendered incapable of condensing any more; the vapours then pass on to the vessel K, and heat that also.

The liquor then that was distilled into the vessel F, was mixed with the liquor of the vessels I and K, then being \* distilled, cohobated and rectified slowly with slacked lime, produced a very subtle penetrating Æther; it is very remarkable, that this, though free from acid, upon mixing it with water, caused a violent ebullition.

An expeditious method of making Nitrous Æther  
by Distillation, without Fire.

(See fig. 5.) Pour six ounces of the most concentrated spirit of nitre, little by little, on eight ounces of rectified spirit of wine, shaking the vessel each time in which the mixture is made.

Then convey it by a long funnel through the opening of the head at C, into the matrafs A; the opening is afterwards secured by a glass stopper; in warm weather this mixture grows hot in five or six minutes, and distills in a stream into the vessel E, and is over in about half an hour. Before the matrafs grows cold, a fresh mixture is put in as above, and so on for five or six times, till there is liquor enough distilled. This liquor being slowly rectified

\* Spirits of wine was used likewise here to condense the vapours; and though the distillation was conducted with a very slow fire, yet the spirits of wine grew very hot. Spirit of wine was likewise used to condense the vapours in the cohobation, but they did not grow hot.

This liquor without cohobation affords Æther, but not in great a quantity.



with flacked lime, makes very fine *Æther*. The spirit of wine, which was put into the vessels E and F to condense the vapours, is so highly charged with *Æther*, that it will separate on washing with water. This spirit of wine is also an exceeding rich *spiritus nitri dulcis*.

What remains in the matrafs contains a quantity of spirits of wine, which may be separated by distillation.

On the Distillation of the Nitrous Acid, see fig. 2.

The quantity that is condensed in water during the distillation of this acid spirit is so small, that it would be scarce worth saving, if it was not to prevent those noxious fumes, which have such an effect on the lungs of the operator, as frequently to make him spit blood.

Water highly charged with these fumes by repeated distillations becomes blue, and retains its colour \*.

I once distilled, in an iron body with a stone head, 30 lb of nitre, with 60 lb of green vitriol, which I had calcined to whiteness, and was obliged to make use of two vessels of water, as in fig. 5, at F and G, to condense the vapours: this water became

\* Oil of vitriol was used in this operation, to set free the acid of nitre; and I found upon trial the fumes condensed in the water to be a pure spirit of nitre: whereas, in the other operation, where calcined vitriol or copperas was used, the fumes contained some acid of salt. This led me to try the common green copperas, and I found it contained a portion of iron united to the acid of salt: whereas the Dantzick copperas or vitriol contains no acid of salt, and therefore is fitter to make an aqua fortis for the refiners use.

blue in one distillation, and continued so for 18 months, till I made use of it.

A great quantity of air was set free from the beginning to the end of the distillation, owing in a great measure to the acid fumes acting on the iron body; for if distilled in a glass or stone vessel, the quantity of air is not near so considerable.

The nitrous fumes condensed in water, in making the spiritus nitri fortis appear to be more acid than the strongest oil of vitriol made use of for the experiments on spirit of salt.

Water is not heated by these fumes, owing probably to the smallest of the quantity which condenses therein.

#### A further application of this new method of Distillation.

In the distillation of the oil of vitriol, a great part of the acid comes over sulphureous, and is very hard to condense; but, by passing it through water, this condensation is easily obtained: however, a greater quantity of water is necessary for this operation than for the spirit of salt, though the water becomes but slightly acid, yet it is greatly sulphureous, and at the same time acquires no heat.

The sulphureous acid obtained by other means, as by distilling the acid of vitriol with mercury, and other substances, is likewise condensable.

Further, this sulphureous acid of vitriol may, by two or three slow rectifications, be deprived of its acid; but it will still retain its penetrating sulphureous gas-like smell.

The vapours which arise in the deflagration of nitre, with charcoal, antimony, &c. commonly called Clyffus, are very hard to condense; but, by making them pass through water, their condensation is thoroughly effected. See fig. 6.

In the rectification of Phosphorus, if water is made use of to condense the vapours, it will become as white as virgin wax, and almost as pliable; which seems to be owing to the water, which prevents its burning.

In the distillation and rectification of the Vitriolic Æther, it is of advantage to make use of spirit of wine to condense the vapours, which otherwise might have been dissipated.

Besides these, a great many other things, too tedious to mention, may be condensed in water, or spirit of wine, to a very great advantage.

I cannot conclude, without mentioning that this new method of distillation bids fair to discover the mercurial and colouring earths of Beccher; for by this method we can condense the most volatile parts of all substances, far better than by any other means.

And I must acknowledge that I received the first hint of it from the common apparatus for reviving mercury from cinnabar.

I am, sir,

Your most obedient servant,

Clerkenwell,  
Nov. 18, 1767.

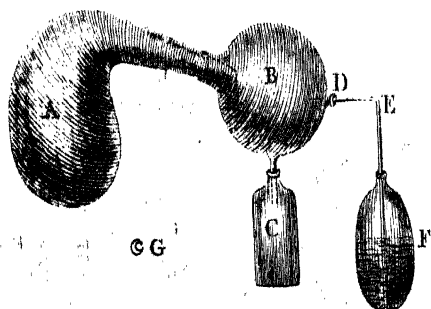
Peter Woulfe.

To John Ellis, Esq;  
in Gray's Inn.

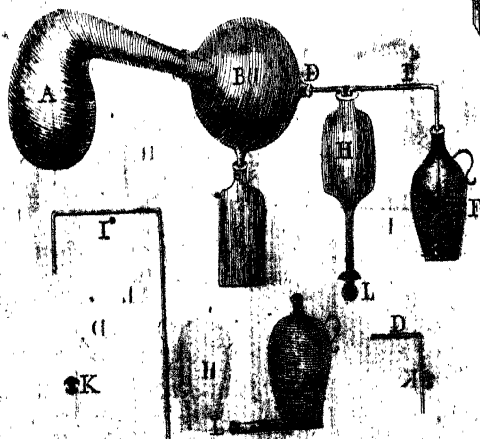
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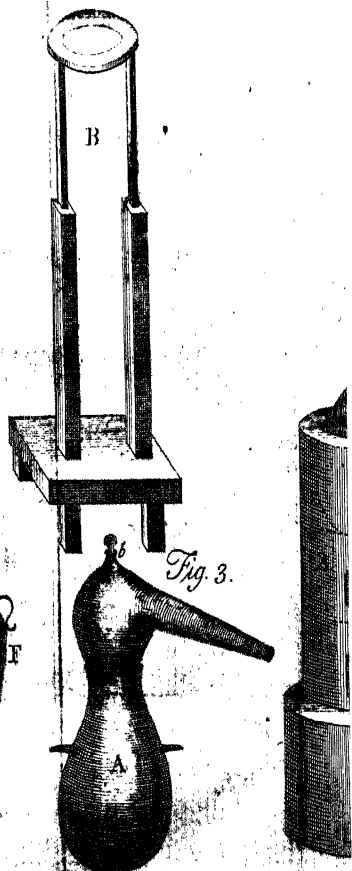
*Fig. 1.*

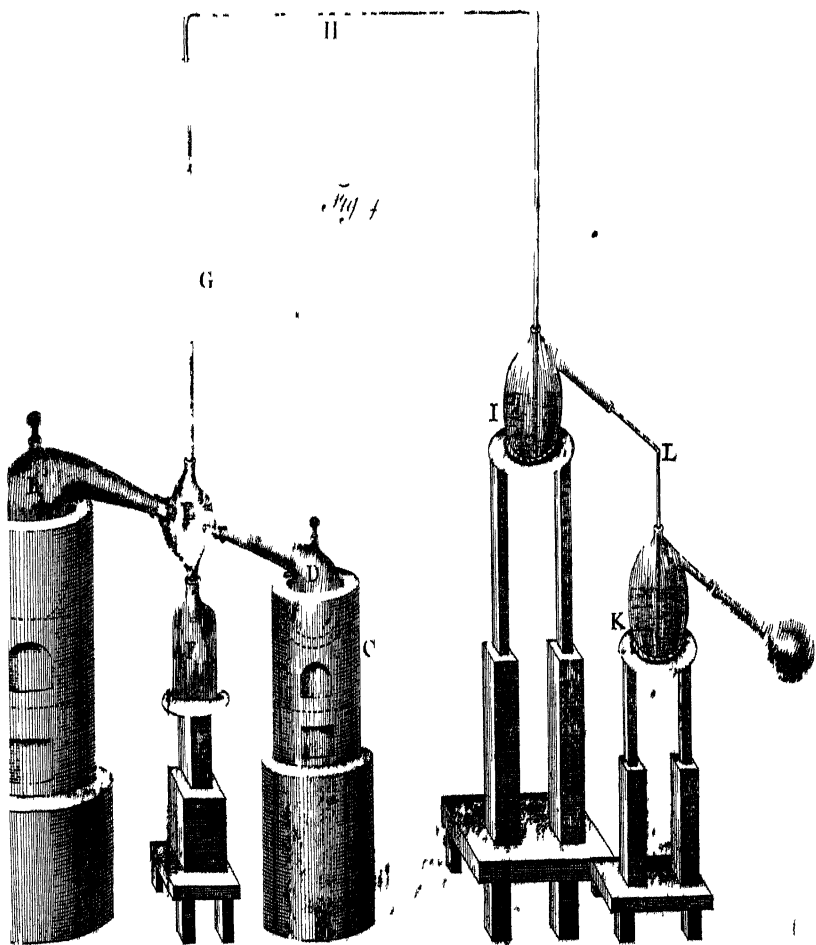


*Fig. 2.*



*Fig. 3.*







Explanation of Plate XXV.

Figure 1.

- A, A glass retort.
  - B, A glass receiver.
  - C, A bottle to receive what distils.
  - F, A glass, or stone vessel with water.
- The recipient B, has a spout at the bottom, which conveys the liquor which distils into the bottle C; at the end there is a spout D.
- E, A crooked glass tube  $\frac{1}{8}$  and  $\frac{1}{2}$  of an inch bore.
  - G, A cork with a semicircular notch to stop the bottle F.

Figure 2.

- A, A glass retort.
- B, A glass receiver.
- C, A bottle to receive the distilled liquor.
- HH, Glass or stone vessels, with glass stoppers, ground and fitted to LL.
- F, A stone bottle with water.
- D, A crooked tube, as at E, fig. 1.
- I, Another crooked tube.
- K, A cork, with two semicircular notches to fit the crooked tubes to the vessel H.

Figure 3.

- A, An iron body with a stone head, which has a stopper at b.
- B, A stand to support the receivers and bottles.

Figure 4.

- A, The furnace, in which is placed the retort B.
- 2
- B, A



- B, A glass tubulated retort, which is to be coated with loam up to B.
- C, Another furnace.
- D, A tubulated retort, fixed in a vessel with sand.
- E, A stone vessel, wherein the vapours of B and D are combined together.
- F, A bottle to receive the liquor which distils.
- G, A large tube fitted to E, about  $\frac{3}{4}$  inch bore.
- H, A crooked pipe about  $\frac{1}{4}$  inch bore.
- I and K, glass vessels containing spirits of wine.
- L, A crooked glass tube.

Plate XXVI.

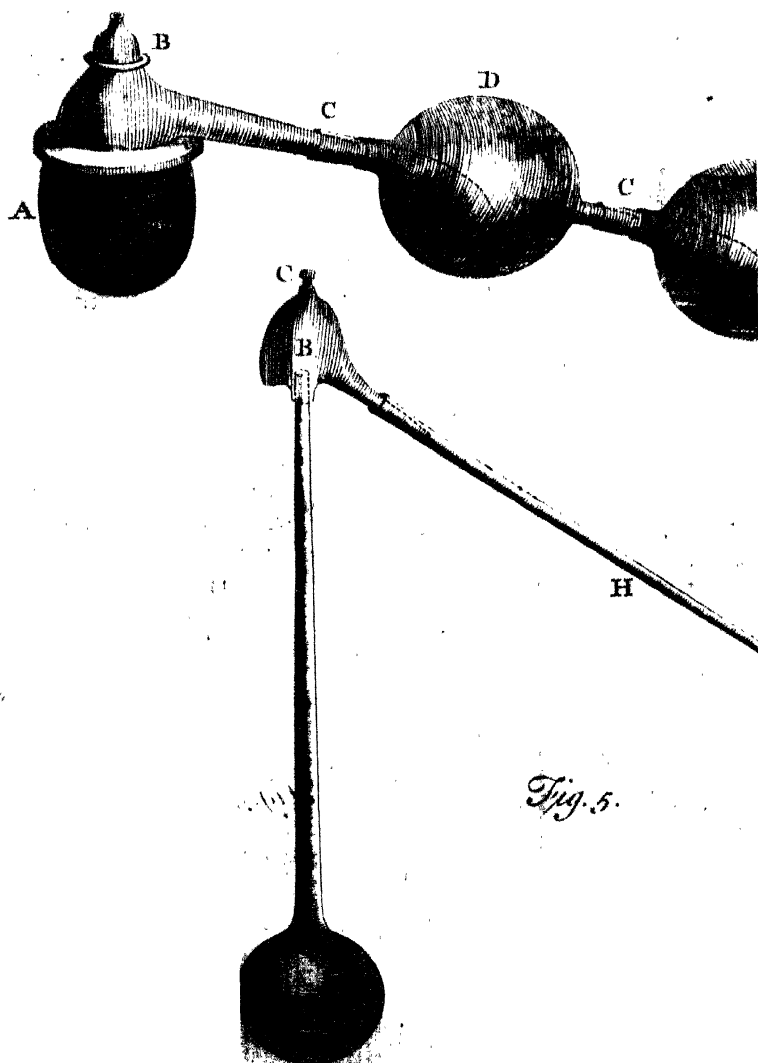
Figure 5.

- A, A glass matrafs about  $4\frac{1}{2}$  feet high.
- B, A glass head, with a spout and glass stopper C.
- H, A glass tube.
- P, The receiver.
- E, The bottle to receive the liquor which distils.
- F and G, Glass vessels containing spirits of wine.
- H H, crooked tubes.

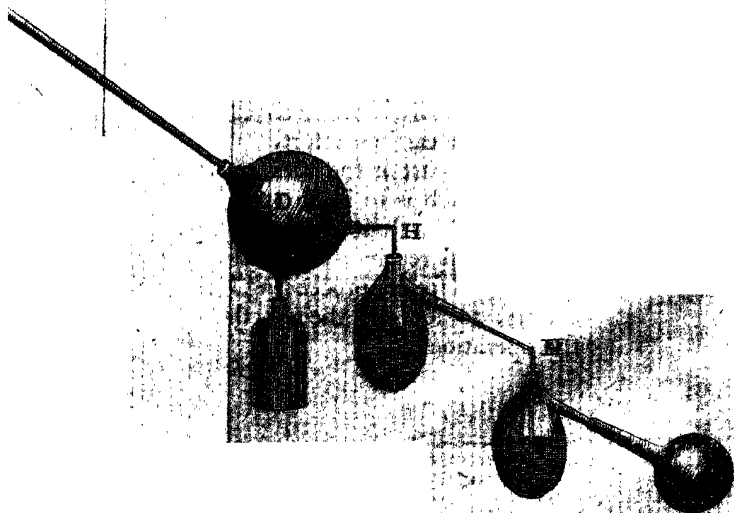
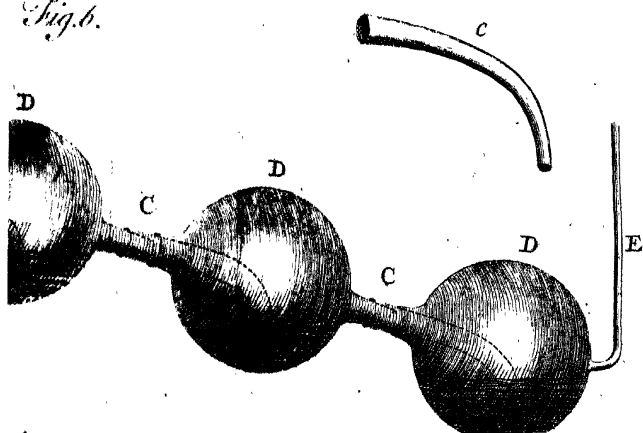
Figure 6.

- A, An iron or earthen retort.
- B, The upper part of the retort, with an opening at top, which is to be stopped occasionally.
- CCCC, Crooked stone pipes.
- DDDD, Glass receivers, containing water.
- E, A crooked spout, proceeding from the last receivers, to let out the air that is set free in the operation.





*Fig. 6.*





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A. N  
I N D E X  
T O T H E  
Fifty-Seventh V O L U M E  
O F T H E  
*Philosophical Transactions.*

For the Y E A R 1767.

A.

*ABDUL*, Roobin, a Sooleo pilot, his observation about the islands on the N. E. coast of Borneo, p. 394. Useful to navigators, 397.

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